SCIENCE

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE NAVAL OBSERVATORY REPORT.

THE first impression of our readers on glancing at the annual report of the Naval Observatory, of which the essential portions are reprinted in our columns, will be one of pleasure that the head of the Observatory has deemed it appropriate to review the conclusions of the board of visitors appointed by the Secretary of the Navy in 1899. But pleasure will be changed to disappointment at what the review omits. It maintains with an energy of expression quite unusual in an official paper that a majority of the board was hostile to the Observatory; that it failed to report on the main points submitted, and that the measures which it proposed are 'preposterous' and 'only ridiculous.' But not a word is said in reply to the destructive criticism of the management of the institution 'during almost the entire period of its existence,' which is one of the most important features of the report. Had these strictures been now heard of for the first time some reason might have been found for ignoring them. But they are little more than an echo of strictures emanating from or endorsed by Secretaries of the Navy, the National Academy of Sciences, at least one distinguished naval offi-

cer, and every eminent astronomer in the country who has ever expressed an opinion on the subject. Utterances by such authorities are surely worthy of being honored by a refutation. We wish to facilitate in every way the task of making the most effective refutation possible, and this we can best do by stating the substance of the criticisms as clearly and forcibly as we can, without implying any endorsement of them until we hear the other side.

The only rational object that the nation can have in supporting a great public observatory is the continuous making of those astronomical observations which require to be prosecuted on a uniform plan for long intervals of time, with a force larger than private observatories can ordinarily command, and with a persistence more long-continued than they can be expected to exhibit. The first requirement of all is a comprehensive plan of work, devised by the best authorities at the command of the nation, and based on the latest aspects of astronomical science. This plan should be pursued without change except as improvements are suggested by the advance of research. It should be dependent on the life or temporary opinions of no individual, and should have a form of public support which will secure adherence to it.

The criticism directed against the Observatory is that its published observations and reports show little or nothing of this kind, and that neither permanence of purpose nor unity of object can be discerned in them. Much that we find in the published volumes looks like a collection of individual works of every degree of ability from the highest to the lowest, which have the appearance of being initiated by the worker himself, abandoned when he deemed it best to do so, and only now and then controlled by the guidance of any higher authority. Even in the case of those observations in which continuity from year to year can be best traced, gaps and unexplained changes and omissions occur through the whole history of the Observatory, for most of which no explanation is found, and none can be readily imagined except the varying moods of the observers.

In these points the critics see no evidence of any permanent improvement in the work of the new establishment. The reports for the last eight or ten years show earnest efforts made by this and that astronomer to do this, that and the other, set forth the difficulties encountered in making these efforts, report the success reached in overcoming them, and describe the alterations necessary in instruments and arrangements. Sometimes an effort made one year seems to be continued into the next, and sometimes we hear nothing more of it. It would be tedious to enumerate all the enterprises which figure for a year or two in the annual reports, without any reason being assigned, and then disappear without any explanation. There seems to be as striking an absence of continuity from year to year as there ever was, the work with the prime vertical transit excepted.

It is not denied by the critics that able and industrious astronomers have, from time to time, been attached to the institution, that the qualifications of the working force are still excellent, and that much good

work has been done by it; but it is also claimed that, for the most part, this work could have been equally well or better done at universities and private institutions, and that it is mixed up with so much indifferent work that the separation of the two is difficult. It is also admitted that much of the work compares favorably with that done by professors and students at the various colleges and universities of the country. But the critics do not see why the government should establish a great institution and give men commissions in the navy to enable them to do work which professors and students are doing at universities and which is of no special naval use.

In framing a reply to this criticism we trust that several other points which invite adverse criticism will be cleared up to the pleasure of all. One is especially worthy of mention because we think, if anything can be said to correct the unfavorable impression which it makes, it ought to be said authoritatively and as speedily as possible. Among the important subjects of scientific observation and investigation today is terrestrial magnetism. The good policy of the observatory in entering upon a field already occupied by another bureau of the government may be open to question, but this is aside from the main point. One of the best-equipped magnetic observatories was established and, we believe, observations made for a year or two. We learn from recent reports that the observations were suspended and the department discontinued, because the action of the electric current from a trolley line in the neighborhood so disturbed the instruments as to deprive the results of all scientific value. Now, we hope the head of the establishment will explain how it happened that one of the finest magnetic observatories in existence was established 400 yards from a trolley line, when a simple computation of the effect of the electric current by a magnetic expert would have shown that its inductive effect would be destructive to the results, and, unless we are mistaken as to dates, after experience elsewhere had shown that the presence of such a line within a mile or even a mile and a half of a magnetic station was fatal to the usefulness of its work. All good wishers of the Observatory would be glad of a proof that this happened through no fault of its administration.

We also suggest that something be said to remove the unfavorable impression made by the unfortunate experience of the institution with its instruments from the beginning of its history. How many costly instruments, supposed to be of the finest quality, have been procured, tested, reconstructed, found wanting and suffered to disappear from view, we cannot say without an examination of the records. But the recent easily accessible reports and documents throw some light on the later history of the subject.

The great 26-inch telescope, constructed about 1874, was found to be so defective that, on moving into the new Observatory, what was substantially a new instrument was constructed. We believe that nothing but the object-glass and, perhaps, some accessories of the old instrument were retained. Whose fault was this?

Perhaps the most necessary and important instrument the Observatory ever had was the 9-inch transit circle, constructed, we believe, some time in the sixties. It was found so defective in stability as to greatly impair the value of its results. Attempts to correct the defect proved unavailing, so radical changes were made in the instrument at a cost, if we mistake not, which could not have been greatly short of the original price. No sooner was it remounted at the new location than defects were found in its performance which, according to the annual reports, it took another year to remedy. In 1896 it is reported as again taken down for a period of five months in order that further extensive repairs might be made. The report of 1899 gives an elaborate account of renewed efforts during the first six months of the year to improve its performance, concluding with the statement: "In my opinion it should be restored as nearly as possible to its original construction." But this does not end the history. We learn from the present report that the instrument was again undergoing repairs and alterations for the first six months of the last fiscal year, and yet it winds up with the statement that "one of the defects still introduces liabilities to error which, though small, ought not to be tolerated in a modern instrument."

The defects in this instrument naturally led to the desire to have a better one. Accordingly, among the appropriations made for the new establishment was one of $10,000 for a meridian circle. This instrument was completed and mounted in 1897. We learn from the report before us that it exhibits a peculiarity of a kind so singular that it should interest physicists as well as astronomers. Its horizontal pointing changes so rapidly with the temperature that, by the aid of the delicate meridian mark, a variation produced by a change of a single degree in the temperature must be an easily measurable quantity. Consequently the instrument could be used as a fairly delicate thermometer. It is hardly necessary to add that observations with such an instrument cannot have much scientific value until the defect is corrected, and that the policy of the observers in not attempting serious work with it cannot be questioned.

The new 12-in. equatorial has proved unsatisfactory. There are also intimations that something is wrong with the prime vertical transit, and, altogether, the impression made on the reader is that, after seven years of effort to equip the Observatory with the best instruments, it is doubtful whether a single one of real importance, except the great telescope, is in order for first-class work. We hope something can be said to explain this history, which is without example. The Greenwich transit circle has been in continuous use for half a century except during a brief period of its early history, when some alteration was made in it. The small repairs which it has since required have caused no interruption in the continuity of its work. The instruments of the Pulkova Observatory have, for the most part, been in use since 1840, and it is believed that their results are to-day among the best attainable in astronomy.

Were only a single instrument involved in the case of our own Observatory, it might

well be set down to unavoidable misfortune. But so long-continued a history leads one to infer a cause, and we should be glad to see something said to efface the impression that the administration is at fault.

In the way of assistance we suggest a few other questions as worthy of careful consideration. It is understood that, when the Astrophysical Society of America chose its committee to confer with the Secretary of the Navy on the question of the Observatory, especial pains were taken to select men who had never been known to express any opinion on the subject. We are quite sure that the Society would like the Superintendent of the Observatory to state how he knows that in making the selection, it was the victim of misplaced confidence, and that the members were 'known to be inspired by a hostility to its organization.'

A cognate point is this: The membership of the Society includes a respectable and influential number of members who have been connected with the Observatory at one time or another in various capacities and who would be its natural defenders. Would not some of them have objected to the appointment of a hostile committee?

The greater number of our astronomers are the mildest of men, and glad to see their science promoted in every way. How does it happen that they, as a class, are moved by 'animosity' toward a national institution for promoting their science?

We wish also to discover every possible justification for the claim that "no person, no matter how eminent he may be in science, can pretend to be a friend of the Observatory or of science while attacking its organization." This is preceded by the statement that "the number of observations made at the old and new Observatories kept pace with those made at Greenwich." If the report had said that the Observatory during the past ten years, with less than half the personnel of Greenwich, had, on the average, done nearly one-half as much work, the critics might have inquired in reply whether this was not a slight exaggeration. They might also have inquired whether it was not desirable to take account of the quality as well as the quantity of the work, and whether in that respect the observations described in the annual reports of the last ten years could compare even with one-third of the work done at Greenwich. But when we find the head of the Observatory seriously believing that some comparison can be instituted between the output of the two observatories we see that he has, from his own point of view, just cause of resentment against the critics of the institution, and feel encouraged to believe that, when he has ascertained the facts, he will, as an act of justice, fair play and public policy, admit that the 'prejudices and animosities' of the astronomers have better grounds than he had supposed.

REPORT OF THE SUPERINTENDENT OF THE NAVAL OBSERVATORY.

[WE regret that we have room only for such portions of this interesting report as relate to astronomical work and the report of the board of visitors.]

DEPARTMENT OF ASTRONOMICAL OBSERVATIONS.

The work of the year in this department may be summarized as follows:

Professor William Harkness, who retired from active service on December 15, 1899, was succeeded in the astronomical directorship by Professor Stimson J. Brown, the senior professor of mathematics on duty at the Observatory. Professor Brown's first duty was as member of a board of examiners, to fill the vacancy caused by the retirement of Professor Harkness, by the selection of a suitable candidate for appointment as professor of mathematics in the navy. The labors of this board, of which Professor William Hendrickson, U. S. N., was senior member, resulted in the selection and appointment of Professor W. S. Eichelberger. In this connection it is pertinent to observe that the present condition of the astronomical staff of the Observatory is highly satisfactory, and gives promise for the future. The vacancies, caused by the rapid successive retirement of the older professors, have all been filled by young men of marked ability, and the Observatory has followed its traditional policy in this respect, and has every reason to anticipate a renewed activity and a period of harmony, industry and usefulness.

All the astronomical instruments of the Observatory have been steadily and continuously in use during the year on every clear night and day.

The XXVI.-inch equatorial telescope has been employed on the observation of difficult double stars, the satellites of the planets and the measurement of their disks, and on other objects suitable to an instrument of its power. This instrument was in charge of Professor Brown, assisted by Professor T. J. J. See, until December 15th, and has since been in charge of Professor See.

In the month of October, during the hazy weather common at that season, the images of celestial bodies were marked by great steadiness in the field of view, unusual at other times. Owing to this favorable condition, Professor See noticed on several occasions faint markings on the disk of Neptune, similar to the equatorial belts of the planets Jupiter and Saturn. The fine definition at this time was ascribed to the selective absorption of the haze caused by the smoke of forest fires, and an attempt has been made by Professor See and Mr. George H. Peters, the photographer, to reproduce artificially the same conditions by the use of a color screen. The idea of this application to the telescope was not wholly new, although it was believed to be so at the time. It is too early to speak definitely on the result of these experiments, except to say that the general effect of the use of a screen composed of two thin sheets of plate-glass, the space between them being filled with a solution of chloride of copper and picric acid, or a solution of bichromate or chromate of potash, seems to have the effect of absorbing the blue rays and removing the halo which surrounds bright objects in the field of a large achromatic telescope, and the image appears somewhat steadied. The color screen is now in use in observations of the diameters of the planets and measurable satellites.

During the year the double floor, which was appropriated for last year, designed to obviate the difficulty experienced from currents of warm air rising from the basement, has been finished, and answers every expectation, giving highly satisfactory results.

The IX.-inch transit circle was undergoing its periodical repairs and general overhauling from the beginning of the fiscal year until January 20, 1900, during which time the routine work of the sun, moon and planets was carried on with the new VI.-inch steel transit circle. Since the resumption of the regular work on the IX.-inch circle, Professor Skinner has had charge of the zone observations and Professor Eichelberger of the routine sun, moon and planet work, while the VI.-inch circle, in charge of

Professor Updegraff, has been under strict investigation, with a view to determining the causes of several faults made apparent by the routine work and probably unavoidable in a new instrument.

In the meridian work a total of 1,114 observations of zone stars and a total of 3,105 observations in the sun, moon and planet work have been made during the year.

The XII.-inch equatorial has been used for observations of comets, asteroids and other miscellaneous objects, and will soon undergo some extensive repairs and alterations.

Assistant astronomer George A. Hill has devoted the prime vertical transit to observations of α Lyræ and a small list of suitable stars in the investigation of the variation of latitude and the constants of nutation and aberration, and the same observer has used the new V.-inch steel alt-azimuth instrument principally as a zenith telescope for observations for latitude by Talcott's method, and for the declinations of stars of the American Ephemeris.

The 40-foot photoheliograph has been used in making daily photographs of the sun whenever the weather permitted, except after April 2d, during preparations for the observations of the total eclipse of the sun on May 28th.

By an appropriation of $5,000 the Observatory was able to undertake observations of the total eclipse of the sun on the date above mentioned. Two parties were sent into the field, and attention is respectfully invited to the detailed report of the astronomical director on this subject, and also to the recommendation for an appropriation to cover the expenses of a similar expedition this year, in the eclipse of May 17, 1901, which is approved and indorsed.

Preparations are now in progress for installing at the Observatory a new standard clock which shall be entirely freed from the influences of changes of temperature, and the recommendations of the astronomical director on this subject are approved.

The work of bringing up the publications of observations to date, which has been in arrears for several years, is being vigorously pushed, and the issue of the first volume of the new series of Washington observations, commencing with the first year of the Observatory's work on the present site, will soon occur, to be followed by the succeeding volumes in rapid succession. The delay in the publication of its work has been made a reproach to the Observatory, although the causes of this delay have been reasonable and unavoidable, but the most earnest and determined effort is being made to relieve the Observatory of a condition which has been used to its disadvantage in outside criticism.

The report of the astronomical director is herewith transmitted in full.

The department of magnetism has been discontinued, as noted in the last report of the superintendent. The buildings and instruments are, however, maintained in perfect order under the charge of assistant astronomer Theo. I. King. The Georgetown and Tennallytown Electric Railway has already installed a return wire which, it is reasonable to suppose, it intends sooner or later to use. When this road ceases to ground its powerful working currents, an attempt will be made to resume magnetic observations at the Observatory. The result is dubious. Moreover, the Observatory is to a lesser extent within the influence of the Chevy Chase road, which would continue to affect observations.

BOARD OF VISITORS.

As noted in my last report, and for the first time in its history, a board of visitors was appointed which visited the Observatory in June and again in September, remaining a few days at each visit. As the report of the board, which was not made

available to the Observatory until long after its substance had appeared in the public prints, was in the nature of an attack on the Observatory, I trust that it will not be out of place, in the report of the Superintendent, to touch upon its salient features.

The board was unfortunate in having the support of several zealous newspaper writers, by whom it has been made to appear, and I think that it is generally believed in the scientific world and by that part of the public which takes an interest in scientific affairs, that the presence of the board was in the nature of a visitation distasteful to the Observatory and inflicted against its will. As the Observatory has thus been made to appear in an odious light, I take the liberty of reminding the Bureau that not only the original conception of the board of visitors was a suggestion of the Superintendent, but the selection of the three astronomical members was concurred in by him because they were already the members of a self-constituted committee to conduct a gratuitous investigation of the Observatory, and known to be inspired by a hostility to its organization. It was hoped that by placing them in a position of official responsibility an impartial judgment could be obtained.

A criticism of the report may be very briefly summarized. The board made no examination or inspection of the Observatory except in the most casual way. To quote its own language, "Owing to the lack of printed material representing the recent work of the Naval Observatory, the board of visitors finds it practically impossible to form a satisfactory opinion of that work without devoting to the task an inadmissible amount of time and labor." Nevertheless, and without such an opinion, the board did not hesitate to recommend the most sweeping changes in the present organization, and it is noticeable that the report of the board deals exclusively with the question of reorganization, while that question had not been officially presented to it at all. As a guide to the board the following letter of instructions was addressed to it by the Department:

NAVY DEPARTMENT,
WASHINGTON, JUNE 28, 1899.

SIR: I have the honor to enclose you herewith a memorandum making certain suggestions which the Department wishes the board, of which you are the chairman, to have in mind in making an examination of the Naval Observatory. This memorandum is not intended to finally limit the investigations of the board, but to convey certain suggestions in regard to which the Department desires to be informed, and which, it is believed, may assist the board in its work.

JOHN D. LONG, *Secretary.*

Hon. WM. E. CHANDLER,
Chairman Board of Visitors, Naval Observatory, Washington, D. C.

[The points covered by the enclosure, which is too long for reproduction here, are meridian observations of the sun, moon and planets, spectroscopy and photography, chronometers, the Magnetic Observatory and the publications of the Observatory.]

Not one of the subjects proposed in the above memorandum received the attention of the board or was even mentioned by it. The whole report is taken up with the proposed reorganization, a subject which it had not been asked to consider at all, and for which it could only find a general authorization in the concluding paragraph of the Department's letter.

Notwithstanding that the board had no time to form an opinion as to the condition of work at the Observatory, the conclusion upon which it bases a recommendation for a revolutionary change of organization is found in the charge that the output, in published results, is not commensurate with an extravagant annual outlay. In other words, the board lays particu'ar stress upon the delay in the publication of the annual volume, and bases its whole finding upon that fact.

The board makes a comparison between the Naval Observatory and the observatories at Greenwich and Harvard, selecting the former because it is supposed to cover nearly the same field, and the latter because it also undertakes large pieces of routine work which are beyond the reach of smaller observatories, because the resources of Harvard approach more nearly than do any other those of the Naval Observatory, and because all the details of the Harvard Observatory were readily available, its director being a member of the board.

I ask the attention of the Bureau to the following table, which gives an exact comparison of the three observatories chosen by the board, and which, almost without elucidation, confutes the sole and only charge made by the board against the efficiency of the Observatory upon which charge the only recommendation of the board rests.

Comparison of number and cost of scientific personnel of Greenwich, Harvard, and Naval observatories, previous to July 1, 1900:

GREENWICH OBSERVATORY.

Scientific personnel.	No.	Average.	Total.
Director	1	$5,000	$5,000
Chief assistants	2	2,550	5,100
First-class assistants	4	2,000	8,000
Second-class assistants	4	1,200	4,800
Magnetic superintendent	1	1,700	1,700
Assistant magnetic superintendent	1	1,000	1,000
Computers	23	325	7,475
Total	36	.....	$33,075

HARVARD COLLEGE OBSERVATORY.

Director	1	$5,000	$5,000
Astronomers	6	2,000	12,000
Assistant astronomers	13	900	11,700
Computers	18	600	10,800
Total	38	.....	$39,500

UNITED STATES NAVAL ACADEMY.

Astronomical director	1	$3,500	$3,500
Other professors of mathematics	4	2,700	10,800
Assistant astronomers	3	1,900	5,600
Computers	6	1,200	7,200
Photographer	1	1,200	1,200
Total	15	....	$28,300

A comparison of the figures in this table shows that the number of astronomers employed at the Naval Observatory is 5, at Greenwich 3, at Harvard 7. The Naval Observatory has the advantage of Greenwich and decidedly the disadvantage of Harvard. In the matter of assistants, however, the case is different. The total of assistants at Greenwich is 8, at Harvard 13, and at the Observatory 3; while the computers stand, Greenwich 23, Harvard 18, Naval Observatory 6.

In the years 1891 to 1893 the Observatory was removed from the site which it had occupied for nearly fifty years, to its present situation. The delays in building, the labor of dismounting and remounting the instruments, the repairs and alterations of the same and their subsequent installation, the removal of the records and Observatory property, and generally the labor involved in settling in the new place, absorbed the entire time of the whole very limited working force of the Observatory for several years. The publication of results, from this cause and from this cause alone, fell into arrears. The force of the astronomical staff, sufficient for current work in settled times, was totally inadequate to bring up back work when the work had fallen behind. It was simply a physical impossibility to keep up the publications, and to make the current observations and do the necessary work of removal at the same time. This and this alone is the cause of the delay in printing, and when the number of assistants and computers at the Observatory is contrasted with the numbers at Greenwich, and especially at Harvard, the delay is reasonable on its face. These facts were represented to the board, but are nowhere given in its report. The board, on the contrary, evaded and concealed them. It might have reasonably been shown that during the whole of this transition period current work was practically uninterrupted, and that the number of observations made at the old and new Observatories kept pace

with those made at Greenwich. But "owing to the lack of printed material the board found it impossible to form a satisfactory opinion upon that work, without devoting to the task an inadmissible amount of time and labor." The facts were laid before the board and ignored by it. They would have shown to the credit of the Observatory.

It is needless to say that the figures in the above table are not given by the board in its comparison of the three observatories named as they are given here. The board had to make a case against the Observatory, and the charge of extravagance of outlay in proportion to the output of results had to be substantiated. Accordingly, the whole staff of the Observatory, scientific and lay, is contrasted with that at Harvard, and the numbers are brought to an equality by adding in the artisans and laborers employed about the buildings and grounds, Harvard having practically none. By this process the astronomical personnel of the Observatory is shown to be one in excess of that at Harvard, and the salaries are compared on this basis, greatly to the disadvantage of the Observatory. This enumeration also charges the Observatory with two 'directors' for the same obvious reason. Although the board had no time to form an opinion, it should have learned, if it could have assimilated the information which it received, that the Observatory, like other naval establishments, has but one 'head,' and that the astronomical director of the Observatory is the head of the astronomical department, just as a naval officer is the head of the department of nautical instruments. To charge two 'heads' to the Observatory in order to increase the apparent extravagance of maintenance is clearly a perversion of fact. If the Observatory has two heads, then it has eight heads, one for each department, and including the superintendent.

The attention of the Bureau is asked to a comparison of salaries, shown in the above table, which forms the basis of the board's charge of extravagance. At both Greenwich and Harvard the salary of the director is $5,000. At the Observatory, using the board's own method of comparison, the corresponding salary is $3,500. But, by the ingenious device of a 'dual head,' the board increases it to $4,000 and then doubles it. Naval officers receive their pay, whether they happen to be on duty at the Observatory or elsewhere; but the board chooses to assume that the salary of every professor of mathematics in the navy, active or retired, except two at the Naval Academy, is chargeable to the expenses of the Observatory. It therefore charges in its exhibit the salaries of officers on the retired list, and the salary of one officer, still on the active list, whose connection with the Observatory has long since definitely ceased for cause. By such flimsy expedients as these the expense column is easily swelled. It might be swelled to any amount desired by simply charging against it the salary of any or every living officer of the navy, active or retired, who had ever been on duty at the Observatory. It is in the lower grades particularly, however, that I ask a fair comparison of salaries at the three institutions selected by the board. At Greenwich, for example, computers receive an average of $325 per year, less than one-half the pay of our laborers, and less than the remuneration of any human being doing skilled work in the United States. At Harvard computers receive an average of $600, less than the pay of any person in the United States service. These computers are largely women, who can be got to work for next to nothing. Now the Observatory pays its employees at exactly the same (or in some cases less) rate as in other branches of the government service in corresponding grades. To charge extravagance against

the Observatory because its employees are paid according to a rate fixed by law for the public service at large is clearly disingenuous and tending to mislead.

The whole report of the board is colored by the evident intention of making as strong a case against the Observatory, and in favor of its own plan of reorganization, as possible.

On page 6 the board objects to transferring responsibility of direction from the astronomical director to a committee, and then, on pages 7 and 8, recommends that the board of visitors 'shall prepare and submit to the Secretary of the Navy regulations prescribing the scope of the astronomical and other researches of the Naval Observatory and the duties of its staff with reference thereto.' In other words, the duties which should belong to the astronomical director, if he is to be held responsible for the astronomical work of the Observatory, are to be transferred to the board of visitors. Furthermore, on page 14, it is proposed that the board of visitors shall have power to make 'necessary changes,' apparently not only in the work, but also in the personnel.

On page 8 objection is made to retiring astronomers at 62 years of age, as is now done, and on page 6 it is specifically recommended that under the proposed new arrangement the astronomical staff of the Naval Observatory are to 'hold their offices until their successors are appointed,' or, in other words, they are to have no tenure of office at all, and to be liable to dismissal at any moment.

The pay table on page 9 proposes to give the astronomical director $6,000 a year, and the director of the Nautical Almanac, $5,000, making a total of $11,000 a year, for the same duties which were then performed by Professor Harkness for $3,500 a year. In view of the distinct charge of extravagance against the present administration, this proposal is only ludicrous. It is difficult to believe that it was intended to be taken seriously.

On page 12 it is asserted that the 9-inch transit circle was not then in use, although the board knew that it was simply undergoing periodical temporary repairs, and would be brought into use again as soon as they were completed.

On page 13 it is asserted that none of the directors of the Greenwich or Harvard College observatories 'have ever resigned to accept positions elsewhere,' but the author of the report forgets to mention that one of the Greenwich astronomers royal did resign.

On page 15 it is indirectly stated that when an observer is not actually observing he is only doing clerical work, which is untrue. The reduction of astronomical observations is not clerical work within the ordinary meaning of that phrase.

The statement on page 30 respecting the personnel of the Observatory board established by Rear-Admiral John Rogers is wholly erroneous. That board comprehended the entire astronomical staff of the Observatory.

In the list of professors of mathematics, given on page 41, Professor H. M. Paul is stated to be attached to the Naval Observatory, when in fact he is attached to the Bureau of Yards and Docks.

An historical sketch of the Naval Observatory, written by an individual member of the board, with the avowed purpose of showing that the system under which the Observatory has been administered since its foundation is entirely defective, is appended to the report. It is a sufficient criticism of this sketch to say that a system which, from a most modest beginning, has built up one of the few great astronomical institutions of the world, and which has produced the two most eminent living astronomers in America, can not be wholly bad.

It is particularly worthy of note that the board actually proposes to remove the affairs of the Observatory from Government control. The irresponsible governing authority of a public institution is to be 'a board of visitors independent of Government control, but having power to make necessary changes.' With the exception of this preposterous proposal, I will dismiss the plan of reorganization proposed by the board with the simple statement that subsequent events have made it impracticable, or at least highly unlikely, to be seriously considered now. To apply it at the present time would be, not to build up, but to tear down.

A determined effort was made by the board to prevent, if possible, the appointment of new men to fill the vacancies created by the retirement of the older professors of mathematics at the Observatory. New offices were created in the board's plan of reorganization, at enormous salaries. Notwithstanding the efforts of the board to prevent it, these vacancies have all been filled in line with the traditional policy of the Observatory, which has always been to take for its staff young men of promise whose career was before them, in contrast with the plan, recommended by the board, of appointing at once to high office men whose scientific reputation was already established, and whose prejudices and animosities were mature and confirmed.

I have ventured to touch in brief upon the report of the board of visitors in a manner which I trust does not transcend the legal limitations of this report, because, while the Observatory has been made the object of adverse criticism in the report, the friends and supporters of the board have not hesitated to assail it in attacks in the public prints, some of which have been extremely abusive and obviously dictated by pure malice, and with the knowledge that a reply in kind was impossible. This report furnishes the only means of replying to these attacks, which are and have been perennial, culminating in force whenever, as in the present instance, circumstances seemed to justify a reasonable anticipation of success.

The Observatory staff is now complete. The excuse afforded by the retirement of the older professors of mathematics no longer exists. For the present, at least, no person, no matter how eminent he may be in science, can pretend to be a friend of the Observatory or of science while attacking its organization. I do not mean to say that the Observatory should be exempt from criticism; but such criticism, in order to be of any benefit, must be made in a spirit of fairness, and not in a spirit of animosity. The Observatory invited such criticism from the recent board of visitors, and its invitation was slighted.

The Observatory is making an earnest and honest effort to correct faults which have, in most cases, arisen from circumstances not wholly within its own control, as the board very well knew, but omitted to point out. It is only reasonable to ask that it be allowed a fair field for its efforts.

The experience with the recent board of visitors was not such as to encourage the hope that much good can be gained by a repetition of the experiment. Nevertheless, I recommend that a board of visitors be appointed from time to time, as may be convenient to the Department, not to act in an arbitrary and irresponsible capacity of authority, 'free of government control,' as recommended by the late board of visitors, but to give the Observatory the benefit of its counsel and to give the scientific world an insight into the actual workings of the institution.

[The following extracts from the report of the Astronomical Director refer to the instruments whose work is not summed up in the preceding report of the Superintendent.]

THE 6-INCH STEEL TRANSIT CIRCLE.

[Professor Milton Updegraff, U. S. N., in charge.]

* * * * * *

This instrument is made entirely of steel and mounted very solidly, so that the constants might reasonably be expected to be unusually steady. In the case of the level and collimation this expectation has been realized, but not in the case of the azimuth. Both the collimation and the level are remarkably uniform, excepting that on one or two occasions the latter has shown slight though anomalous changes. A series of experiments made in November, 1899, showed that the variation of the azimuth keeps pace regularly with the variation of the temperature of the steel in the microscope bearers on which the pivots rest. This variation is not, however, a linear function of the temperature, but is less per degree at low than at high temperatures. The change of azimuth per degree of temperature (Fahrenheit) during the earlier part of its use seems to have been 0^s.032. Since the instrument is provided with a meridian mark to the north, this variation of the azimuth has been productive principally of inconvenience rather than inaccurate work; but the change is too great to make the instrument a suitable one for fundamental work. The instrument will in the near future be entirely dismounted and a careful investigation made in the hope of finding the source of this variation.

THE PRIME VERTICAL TRANSIT INSTRUMENT.

[Assistant Astronomer George A. Hill, in charge.]

The work on this instrument has been devoted to observations of α Lyræ and a small list of suitable stars for the purpose of investigating the variation of latitude and the constants of nutation and aberration. The number of observations is as follows:

α Lyræ	89
Other stars	75

A careful investigation of the level-constant, with a special reference to the coincidence and parallelism of the axis of the pivots, is in progress, but the material is not yet sufficient to determine whether any defect exists in regard to this which is of sufficient magnitude to affect the observations.

THE 5-INCH STEEL ALT-AZIMUTH INSTRUMENT.

[Assistant Astronomer George A. Hill, in charge.]

This instrument is used principally as a zenith telescope for observations of latitude by Talcott's method and the declinations of the stars of the American Ephemeris. Of the former there have been made 86 observations of α Lyræ and three of miscellaneous stars, which are reduced up to date, and the results in January, 1900, together with the material from the prime vertical instrument, sent to Professor Albrecht. Of the declinations of Ephemeris stars 330 observations have been made, and the reductions are completed up to the end of the calendar year.

The results have not yet been subjected to sufficient study to determine the quality of the instrument for obtaining absolute declinations, for which purpose the general form of the mounting is admirably adapted. When its usefulness in this field is established I propose to separate the work of the two instruments and employ more observers in this important line of observation. I have not thought it expedient to attempt this until the arrears of publication are brought up—a task on which all the available force of the Observatory is now concentrated.

THE 40-FOOT PHOTOHELIOGRAPH.

[Photographer George H. Peters, in charge.]

The series of daily photographs of the sun has been continued, weather permitting, except after April 2d, when they were interrupted by the preparations for the observations of the total eclipse in May. Nega-

tives were obtained on 81 days, showing sun spots on 18, distributed as follows : Two in September, two in December, five in January, five in February, three in March and one in April. Visual observations of the sun in May indicate the same low state of solar activity.

STANDARD CLOCK.

Preparations are now in progress for installing a standard clock in a hermetically sealed case to be kept in a double-wall chamber at a constant temperature. The device for keeping the temperature constant is entirely similar to that now employed so successfully in the temperature room for testing chronometers. I consider this subject as one of the most important for the future of the fundamental work of the Observatory, and no pains or expense ought to be spared in securing the best possible performance of a standard clock under the conditions above described. I regret that the preparations for the eclipse so completely occupied the resources of the Observatory that this important matter has had to be laid aside until the present time.

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*AIMS AND METHODS OF STUDY IN NATURAL HISTORY.**

I INVITE your attention to an old but still fruitful topic, namely, the aims and methods of study in natural history. It is a well-worn theme, but one that will retain its interest to the naturalist so long as natural history remains a progressive subject; and I venture to think that it was never more timely than at the present period of intense activity in natural science, of rapid development of new aims and methods, and of continually shifting point of view. How great the changes have been in the last twenty or even ten years is, I dare say, hardly realized by many of the younger generation of naturalists to-day. To appreciate their full extent one must be old enough to have passed his student days in the sixties and seventies, at a time when it was still possible to discuss the truth or error of the evolution theory; when the germ theory of disease was itself no more than a germ; when a gastrula or a karyokinetic figure was a thing to be spoken of with bated breath, but not to be looked upon when there were no oil-immersion-lenses or Abbe illuminators, no automatic microtomes, no ribbon-sections, no chromosomes or centrosomes, no shaking of eggs, no 'taxes' or 'tropisms'; when to adopt the career of a naturalist was to face the imminent prospect of extinction in the struggle with the environment, and to incur the half-admiring, half-contemptuous compassion of one's relatives and friends.

Speaking as I am in the presence of some of those who guided my own first tottering footsteps along the pathway of science, I feel some hesitancy in claiming a place among those veterans of the old guard; but I am nevertheless able to recall days when we had to do without all the things I have mentioned, as well as a good many others, both material and spiritual, that are now considered the very bread of life in the day's work. I will confess, too, that I am old enough to be at times lost in wonder at the child-like serenity with which the modern student will accept many of these matters, which cost such travail of the spirit, and at the distant epoch to which I have referred would have produced a sensation throughout the scientific world. When, for instance, Kleinenberg made the famous declaration 'Es gibt gar kein mittleres Keimblatt' it seemed to us that the sky must fall on such a blasphemy. We have changed all that. Cite those memorable words to-day, at the climax of your cautious discussion of the

* Presidential Address delivered at the annual dinner of the American Society of Naturalists, Baltimore, December 28, 1900.

germ-layer theory, and your *fin de siècle* student merely remarks stolidly, as he reels off a yard or two of ribbon-sections from his Minot microtome, "Of course not; but what is the use of talking about such an antediluvian myth?" It is enough to make Balfour turn in his grave!

I do not propose to review the advance of discovery in recent years, but only to offer a few reflections on the progress of our aims, methods and standpoints, taking as my point of departure Louis Agassiz's delightful little book, entitled 'Methods of Study in Natural History,' published in 1863. In this work we find a clear and simple exposition of the aims and methods of natural history as they appeared to a great naturalist and teacher before the theory of evolution had wrought its wonderful transformation in natural science. We all know that, as far as that theory was concerned, Agassiz ranged himself on the side of a losing cause, believing, to quote his own words, that naturalists were chasing a phantom in their search after some material gradation among created beings such as that theory demanded, though he was constrained to the admission that 'this notion' had a certain fascination for the human mind. I am here concerned with Agassiz's position on this question only in its bearing on his aim and method. It was Agassiz's aim, first, to observe phenomena with all possible accuracy; and, second, to arrange and classify them in order to discover the 'natural affinities' of living things. His method, on the all-importance of which he was never weary of dwelling, was that of his master, Cuvier, *comparison*. "The true method of obtaining independent knowledge" he says "is this very method of Cuvier's—comparison." "The education of a naturalist now consists chiefly in learning how to compare." It was not Agassiz's aim to analyze and explain phenomena, as Darwin was attempting to do. His whole theory of organic creation precluded such an aim; for existing phenomena of life were viewed as the result, not of progressively operating causes, but of special creation, and 'natural affinities' among living things were but the expression of creative thought. It was enough for him to observe, compare and classify. In his work one is everywhere struck with the eager and enthusiastic delight that he took in the facts of natural history for their own sake. The key note of Agassiz's work was, in short, *the love of nature*, and his remarkable success as a teacher was mainly due to his power of inspiring a like enthusiasm in others. Such, in few words, were what seem to me the characteristic features in Agassiz's aim and methods. They may have for us later naturalists a useful lesson, both in their agreement with, and their contrast to, some of the latest *dicta* of modern writers on scientific method.

Leaving aside for the moment the subject of experimental physiology, we may say broadly that the progress of natural history since Agassiz's time has been along three general lines of study, though no very definite line of demarkation between them can be drawn. First came the development of comparative morphology, dominated by Agassiz's method of observation and comparison, but largely inspired by a theory of organic forms that was the very antipode of his own. Here belong the elaborate and exact modern investigations on general and systematic zoology and botany, on geographical and geological distribution and on comparative anatomy and embryology. In all these, a leading motive was to search for natural affinities and to interpret them in accordance with the theory of evolution. It has been a laborious and persistent quest, carried forward on a vast scale; and there is now hardly a corner of the plant or animal kingdom into which it has not been pressed. Its point of departure was primarily given by the comparative anatomy

of existing forms of life, supplemented by that of extinct forms. Almost from the start, however, it was evident that the data derived from those sources were sufficient without the additional evidence afforded by the facts of embryological development. Despite the high degree of validity possessed by the paleontological evidence, the record is, and is likely always to remain, too meager to guide us to the broader results we seek. Without the aid of embryology, comparative anatomy, with all its wealth of data, gives us hardly a hint of some of the most fundamental relations of living things. The high value of the embryological evidence was therefore early recognized; and with the progress of research it played a more and more important rôle in the examination of genealogical problems.

It seems a singular irony of fate that Agassiz, an anti-evolutionist, should have singled out as the most important result of his life-work a discovery in embryology, which, in connection with the generalizations of von Baer and Darwin, was destined to form one of the watchwords of a coming generation of evolutionists. "I have devoted my life to the study of Nature, and yet a single sentence may express all that I have done. I have shown that there is a correspondence between the succession of forms in geological times and the different stages of their growth in the egg—this is all." In another place he urges young students to turn to the study of embryology; for here, he says, lies 'an inexhaustible mine of valuable information—where we shall find the true facts by which to determine the various kinds and different degrees of affinity which animals bear not only to one another, but also to those that have preceded them in past geological times.' How little he foresaw the use which embryologists were soon to make of this principle or the lengths to which they would go in its application. It was in that very year that Fritz Müller published the famous little book entitled 'Facts for Darwin,' which contained the first clear outline of the recapitulation theory and marked the beginning of the embryological search for genealogies, continued with so much ardor by Haeckel, Semper, Claus, Dohrn, Balfour and a hundred others. Many of us have eagerly followed the phases of that long quest or have sought to make our own modest contributions to it. We know how many puzzling problems of comparative morphology it has brought to a solution, how great an impulse was given to the investigation of natural affinities by the formulation of the recapitulation theory by Müller, Haeckel and their followers. I would be the last to question the immense interest and value of the results that have thus been achieved in the field of genealogical inquiry. And yet I believe that when these results, together with those derived from all other sources, are broadly viewed, we are constrained to the admission that comparative morphology as a whole has thus far solved only minor problems of descent, and that naturalists as a body are beginning to turn their attention in other directions. Let any one who doubts this compare the present attitude of naturalists towards some of the more general problems of descent with that of fifteen or twenty years ago. At that time the burning questions of zoological morphology centered in far-reaching genealogical hypotheses such as the *Gastræa* theory, the *Trochophore* theory, the *Nauplius* theory, the origin of vertebrates, the origin of metamerism, or the derivation of bilateral animals from medusoid or polypoid forms. They still remain questions of very high interest, but they are no longer the leading questions of the day; and we may as well admit the truth that interest in them is beginning to wane, temporarily perhaps, but unmistakably.

It will be worth our while to inquire into the reasons for this.

First, we cannot repress a certain feeling of dissatisfaction at the vagueness of our conclusions regarding many of these major problems. Our knowledge of the anatomy and development of the leading types of life is still very far from complete—indeed, the field before us remains so vast that we may never hope to exhaust its possibilities of research. We have, nevertheless, gained a fairly clear view of the general outlines of the system. But have we reached substantial agreement regarding the natural affinities of the great types? In a few cases, yes; but I think the candid naturalist must also reply, in most cases, no. How is it with that time-honored problem, the origin of vertebrates, in one way the most interesting of all, involving as it does our own remote ancestry? How is it with the origin of annelids or mollusks, of echinoderms, of platodes, of round worms or molluscoids? What are the historical relationships of the higher types to the Cœlenterata, of bilateral to radial forms, or of Metazoa to Protozoa? I dare say most of the morphologists present hold more or less definite views on these questions—if I, for one, am charged with holding such views on the zoological side I shall not defend myself or deny that all these are questions of high interest to me. But have we reached definite conclusions on which we are substantially agreed? I fear that a general discussion of the zoological members of this society would elicit but too emphatic a negative reply, and that a similar symposium of our botanical brethren would not set us a better example of unanimity. I do not doubt that the progress of research will in time bring us much nearer to a definite solution of these great problems; though it lies in the nature of the case, that we can never attain complete certainty. In the mean time, we may as well admit that in the application of the embryological evidence to the broader problems of descent the recapitulation theory has encountered so many difficulties, undergone so many modifications and limitations, that investigators have in a measure wearied of their wanderings through the scholastic mazes of ancestral and secondary characters, of palingenesis and cenogenesis, of primary and adaptive forms and the like, and have sought for new interests and fresh motives of study. This is clearly apparent in the changed character of the more recent papers in embryology, which devote far less attention than those of ten or fifteen years ago to 'genealogische Betrachtungen' that once formed their inevitable climax. The relative decline of interest in genealogical questions is partly due, I think, to a healthy reaction against the inflated speculation into which morphologists have too often allowed themselves to fall; but it is also in large measure a result of the growing feeling that the solution of the broader problems of genealogy still lies so far beyond our reach that we would better turn for a time to the study of questions that lie nearer at hand and are, to say the least, of equal interest and importance.

We here arrive at a consideration of the two other great lines of progress to which I have referred. The first of these includes the modern developments of the cell theory, which have perhaps contributed equally with the evolution theory to the unification of biological knowledge. I need not dwell on the fundamental importance or the fascinating interest of the general results that have been attained in this field. The point on which I would lay emphasis is that investigation in this direction has only in very minor degree been inspired by the evolution theory or influenced by the historical point of view. The study of the cell, whether morphological or physiological, has been inspired by the desire to penetrate more deeply into the mechanism of

the existing living body. It has established a fundamental unity in the organization and modes of activity of living things, but it has thus far taught us little or nothing regarding their origin and progressive transformations. The interest of the results of cell-research, therefore, is of a different kind from that attaching to the genealogical problems of comparative morphology, and the one has grown, in some measure, at the expense of the other.

A no less potent influence has been the rapid infusion of experimental methods into morphological research, which forms the third line of progress in question, and is fast becoming the characteristic feature of latter-day biology; and with this we may briefly regard the far older subject of experimental physiology. When we regard the novelty and importance of the results already attained through these methods, it seems strange that morphologists were so long content to leave them to the almost undisputed monopoly of the physiologists; and I think that zoologists must admit further that, until recently, they have lagged behind the botanists in this regard. It would, however, be wide of the mark to maintain that experimental methods in morphology are a new product of the day. Did not Bacon, in the 'Novum Organum,' urge that living things are especially adapted for experiment, and in the 'Nova Atlantis' even project a scientific institution for experimental researches with reference to the problem of variation?* More than a century before our time Trembley, Bonnet and Spallanzani showed how rich a field lay in the experimental study of regeneration; and Darwin later taught us what a wealth of suggestive results could be drawn from the long-continued experiments of breeders of domestic plants and animals. Nevertheless, it is only very recently that a definite program of experimental morphology has been laid out, and that naturalists have begun to address themselves seriously to the task.

The revival of experimental methods in morphology is only in part due to a reaction against genealogical speculation. It is in at least equal measure due—and here we touch on a point that is vital to my present purpose—to the closer relations that have sprung up between morphology and physiology, and to the *development of comparative methods on the part of physiologists.* Animal physiology, long confined almost exclusively to the study of vertebrates, at last broke away from its earlier traditions and entered upon a new career, in the course of which it amalgamated with morphology. The traditional line between morphology and physiology thus faded away in zoology, as it had earlier done in botany, as naturalists advanced from either side into a neutral zone of inquiry devoted to the physiology of the lower animals and of the cell, to the activities of one-celled organisms, and to experimental studies on regeneration and development, and on cell-morphology; while in the study of habit, instinct, variation and inheritance the psychologist and even the sociologist have made common cause with us. We may well congratulate ourselves on such a solidification of aim and on the accompanying increase in the exactness and order of our method, and this not merely because of the value of the results attained, but in no less degree through the revival of interest in natural history, in the older sense of the word, that has accompanied it. We see the signs of this revival in many directions—in precise and far-reaching inquiries into the habits and instincts of insects and birds, and the life of animal communities; in renewed and more accurate ecological studies on plants and animals of almost every group, in the increasing interest in systematic zoology and botany, in the extended examination of the plankton of inland waters and the sea, in the rapid

* Osborn, 'Greeks to Darwin,' pp, 92, 93.

development of exact statistical methods in the study of variation, and in many other ways, among which we should not forget the mention of the development of courses of instruction in the so-called 'Nature-study,' and the recent appearance of admirable text-books in which anatomical detail is largely—perhaps too largely—subordinated to the older natural history. I think, too, that we have a right in this connection to point to the influence that such associations as this Society have exerted in widening the range of common interests and fostering the spirit of scientific fellowship and cooperation.

With these changes has come a better understanding between the field naturalist and the laboratory morphologist and physiologist, who in earlier days did not always live on the best of terms. I shall never forget the impression made on me many years ago, shortly after returning from a year of study in European laboratories, by a remark made to me in the friendliest spirit by a much older naturalist, who was one of the foremost systematic and field naturalists of his day, and enjoyed a world-wide reputation. "I fear," he said, "that you have been spoiled as a naturalist by this biological craze that seems to be running riot among the younger men. I do not approve of it all." I was hardly in a position to deny the allegation; but candor compels me to own to having had a suspicion that while there may have been a mote in the biological eye, a microscope of sufficient power might possibly have revealed something very like a beam in that of the systematists of the time. However that may have been, it is undeniable that at that period, or a little later, a lack of mutual understanding existed between the field naturalist and the laboratory workers which found expression in a somewhat picturesque exchange of compliments, the former receiving the flattering appellation of the 'Bug-hunters' the latter the ignominious title of the 'Section-cutters,' which on some irreverent lips was even degraded to that of the 'Worm-slicers'! (For the sake of completeness it may be well to add that at a later period the experimental morphologists fared no better, being compelled to go through the world under the stigma of the epithet 'Egg-shakers.') I dare say there was on both sides some justification for these delicate innuendoes. Let us for the sake of argument admit that the section-cutter was not always sure whether he was cutting an *Ornithorhynchus* or a pearly *Nautilus*, and that at times perhaps he did lose sight of out-of-doors natural history and the living organism as he wandered among what Michael Foster called the 'pitfalls of carmine and Canada balsam'; but let us in justice mildly suggest that the bug-hunter, too, like Huxley's celebrated old lady, was sometimes a trifle hazy as to whether the cerebellum was inside or outside the skull, and did not sufficiently examine that hoary problem as to whether the hen came from the egg or the egg from the hen, and by what kind of process. The lapse of time has in truth shown that each had something to learn from the other. The field naturalist came to realize that he could not attain right conclusions in the investigation of the larger problems before him without more thorough studies in anatomy and development. The laboratory morphologist learned better to appreciate the fact that his refined methods of technique are after all but a means toward the better understanding of the living organism and its relation to its environment. On both sides, accordingly, the range of common interests and sympathies was extended; and some of the splendid monographs of recent years bear witness to the value of the results that have flowed from the combination of anatomical, embryological, systematic and ecological research.

But now, in the last place, we encounter in the recent writings of some of the experimental morphologists a singular attitude of mind toward other methods of study and, in particular, toward the comparative method and the historical point of view in biology generally, for it is seriously maintained that the scientific study of organic nature is possible through experiment and through *experiment alone*. That I am not overstating the case will be evident from the following citations from recent utterances by an eminent leader in this field. "The comparative method in morphology," he says, "is in itself not science, but only a preparation for scientific work." Speaking for the self-styled 'rational morphologists' he says, "We have not *a* method of scientific morphology, but *the scientific morphological method*. There is but one productive method, and that is our method." The historical point of view in comparative morphology is of wholly minor value. Even could we accurately determine the ancestral origin of plants and animals—which in point of fact we cannot do—we should still not have solved the real problem—namely, the laws in accordance with which evolution has taken place. The most complete acquaintance with phylogeny would give us only an ancestral portrait-gallery, nothing more than a 'photograph of the problem.' Only through systematic experiment can we unveil the nature and limits of the power of transformation that lies at the root of the evolutionary process.

We may as well admit forthwith that there is a large element of truth in this bold claim, and it is well to recall how prominent a place the experimental evidence of evolution held in Darwin's mind. The history of science shows incontestably that only through experiment, through deliberately calculated and precise alterations in the conditions under which phenomena occur, can we attain the limits of scientific analysis. So long, therefore, as the naturalist limits himself to the study of vital phenomena under natural conditions, he falls short of the highest ideal of scientific investigation. For my part, I am wholly ready to admit that the introduction of experimental methods into morphology is the most momentous step in biological method that has been taken since the introduction of such methods into physiology by Harvey and Haller. As regards the comparative method, I do not overlook the force of the argument that when comprehensive conclusions are attained by the mere elimination of facts that are not common to all the individual cases compared, those conclusions must have a more limited content than the collective data on which they are based, though I suspect that it would not require a very long search to discover a fallacy lurking here. But seriously to maintain that the non-experimental comparative study of nature is not science is an efflorescence of enthusiasm at which one could hardly repress a smile did it not involve so serious a blunder.

Now I certainly shall not undertake such a work of supererogation as a defense of the comparative method in natural history. Moreover, the statements cited were, I believe, intended mainly as a protest against too free genealogical speculation, and perhaps conveyed more than their author really intended. Yet the undoubted truth that they embody is masked by a form of expression so misleading, that even before this audience of naturalists I shall venture to place beside them the words of one or two of those best qualified to pass judgment on scientific method in the domain of physical science, which may rightly claim to be the experimental science *par excellence*. Helmholtz, in a discourse on the relation of natural science to general science, delivered at Heidelberg forty

years ago, said: "It is not enough to be acquainted with the facts; scientific knowledge begins only when their laws and causes are unveiled. Our materials must be worked up by a logical process; and the first step is to connect like with like and to elaborate a general conception embracing them all. Such a conception, as the name implies, takes a number of single facts together, and stands as their representative in our mind. We call it a general conception, or the conception of a genus, when it embraces a number of existing objects; we call it a law when it embraces a series of incidents or occurrences." What is the first example by which Helmholtz illustrated his meaning? It is one drawn not from experimental science, but from comparative anatomy, namely, the correlations of structure shown by the Mammalia. What was Helmholtz's estimate of the historical point of view in biology? Here are his own words, spoken ten years after the appearance of the 'Origin of Species': "The facts of paleontological and embryological evolution were enigmatical wonders as long as each species was regarded as the result of an independent act of creation, and cast a scarcely favorable light on the strange tentative method which was ascribed to the Creator. Darwin has raised all these isolated questions from the condition of a heap of enigmatical wonders to a great consistent system of development, and established definite ideas in the place of such a fanciful hypothesis as, among the first, had occurred to Goethe, respecting the facts of the comparative anatomy and the morphology of plants."

But, the 'rational morphologist' may reply, these words were spoken thirty years ago, and conditions have profoundly changed since Helmholtz passed this too favorable judgment. Let us see. In his address on the 'Principle of Comparison in Physics,' delivered before the German Association of Naturalists and Physicians at Vienna in 1894, that brilliant and versatile mathematical physicist, Ernst Mach, said: "Comparison, as the fundamental condition of communication, is the most powerful inner vital element of science." What is his first illustration of this truth? Again, as in the case of Helmholtz, it is drawn from non-experimental comparative morphology—from comparative anatomy and comparative embryology. "If it is not customary," he continues, "to speak of comparative physics in the same sense that we speak of comparative anatomy, the reason is that in a science of such great experimental activity the attention is turned away too much from *contemplative* element. But, like all other sciences, physics lives and grows by comparison."

It is needless to multiply such statements. Every really rational naturalist must admit that there is but one sane position to adopt, namely, to welcome any and every method by which our knowledge of organic nature may be advanced and unified. No one, I trust, will understand me to advocate the indiscriminate accumulation of facts—for this is not method, but the absence of method. The essence of science is not the accumulation of knowledge, but its organization. Observation and experiment give us our materials, but it is the comparison and correlation of those materials that first build them into the fabric of science. As I regard the matter, it is therefore a reversal of the true standpoint to regard biological classification, in the broadest sense of the term, as no more than a preparation for experiment. Let us, however, admit that our science is entering on a phase in which experimental methods seem destined, and rightly so, to take the leading rank, and that to them we may probably look for the greatest advances that are to be made in years to come. Let us, too, admit that our existing systems of classification, our views

of genealogical relationship, are incomplete, are in many respects still hypothetical and often misleading; that our methods of study have not been sufficiently exact; that a little judicious clipping of the wings of our scientific imagination will render its flight safer, even though it may not soar so high. But let us not depreciate the importance of the comparative study of normal phenomena to which biology already owes so many brilliant triumphs, and which, as we may confidently hope, has still so many future achievements in store. The true aim of the naturalist is to understand the conditions of living forms as they now exist and have existed in the past; but what are these conditions if not the result of an illimitable series of experiments, carried on by nature since life began? Under what other interpretation has the theory of natural selection any meaning? Comparative morphology and physiology but record and coordinate the results of these experiments. The experiments performed in our laboratories but supplement those that have taken place and are always taking place in nature, and their results must be wrought into the same fabric.

One final thought, and I have done. I have said that the key-note of Agassiz's life and work was his love of nature; and in this respect I believe he was typical of the great naturalists of every age. It has of late become the fashion in some quarters to look with a certain condescension on what is styled the 'sentimental side' of natural history, on that keen primary interest in biological phenomena for their own sake, apart from their scientific analysis, that was characteristic of so many of the earlier naturalists. I can but believe that such an attitude shows a lack of insight into the real motives and sources of inspiration of all great observers and discoverers. Every critical analysis of the progress of science leads to a recognition of the vital importance of the imaginative faculty in all research of a high order; and in this regard great masters of creative science, such as Faraday or Darwin, have rightly been placed beside the great masters of creative art. But this faculty is nearly akin to the esthetic sense. Karl Pearson, in his admirable 'Grammar of Science' remarks, "Both works of art and laws of science are the product of the creative imagination, both afford material for the esthetic judgment." Pearson is here referring, it is true, to the sense of beauty and harmony aroused by the discovery and contemplation of natural law. Beyond this, however, we must recognize that there is no more potent spring of scientific research than a lively interest in the facts—in other words, the esthetic satisfaction that lies in the mere observation of natural phenomena. Read the intimate records of the lives of great discoverers in every field of science, and you cannot fail to be struck with this. From this source flows the impulse to analyze by experiment, to correlate by comparison and thus to discover law. The primary impulse of the naturalist is thus given by the love of nature; and I believe that the scientific naturalist should welcome every movement toward the cultivation of general interest in natural history. We may therefore regard it as a happy omen for the future of our science that in every direction we see the signs of increasing interest in field work, in nature-study and in the teaching of natural history in our schools. It would be an evil day for the more advanced and precise study of biology when it came to be regarded as actuated by motives having nothing in common with the love of nature that inspired such men as Darwin, Agassiz, Audubon and Gray; but we need not fear that such a day will come. For my part, I believe that the newer experimental study is better calculated to foster an interest in nature than much of the

minute anatomical and embryological work that has played so great a rôle in the laboratory methods of comparative morphology. These methods were a healthy reaction against the superficial character of much of the earlier work; they form the indispensable basis of all exact and thorough training in biology; but too often in our courses of instruction they have been carried to such a point that the student has lost himself amid anatomical detail of a kind as dry and formal as that of the old-fashioned systematic museum-study. Experimental research is already, I believe, exerting the happiest influence on our methods of teaching by showing how indispensable to a course in comparative morphology is the consideration of physiological phenomena and a study of the living organism.

I cannot better close than with the words that an eminent zoologist—we of this company have not far to seek him—has imagined to be the comment of Aristotle, could he have surveyed some of the aspects of our modern work in biology. "My teaching that the essence of a living being is not what it is made of, or what it does, but why it does it, has been rendered by one of your contemporaries into the statement that life is the continuous adjustment between internal relations and external relations. If this is true, is not the biology which restricts itself to the physical basis and forgets the external world, like your play of 'Hamlet' without the Hamlet? Is not the biological laboratory which leaves out the ocean and the mountains and meadows a monstrous absurdity? Was not the greatest scientific generalization of your times reached independently by two men who were eminent in their familiarity with living things in their homes?" I for one agree with the author of these words that such a comment would be good common sense and therefore good science.

EDMUND B. WILSON.

SCIENTIFIC BOOKS.

Report of the United States Commission of Fish and Fisheries. Part XXV., 1899. By GEORGE M. BOWERS, Commissioner. Washington, Government Printing Office. Pp. clxiii + 397. Plates XXIX + 21.

The contents of this the twenty-fifth report is divided into two portions, of which the first relates to the official and more practical work of the fiscal year, and the second to the special or more scientific work, the preparation of which may have extended over a considerable period.

In speaking of the general condition of the fishery industry, Commissioner Bowers states that the approximate value of the commercial fisheries of the United States in 1899 was $40,000,000, to which the oyster industry contributed about $14,000,000. In comparing the productiveness of the oyster beds of Chesapeake Bay and of Long Island Sound, Commissioner Bowers states that the natural supply of oysters is being exhausted, but that the areas of the sea bottom that are being artificially cultivated are becoming more and more productive. There is sufficient evidence that the increased abundance of cod, in the inshore waters of the New England States, is due to the work of artificial propagation carried on at Gloucester and at Woods Holl. Efforts are being made to rehabilitate the lobster fishery and to devise methods for increasing the number of sturgeon.

Under the direction of Dr. Hugh M. Smith, the Department of Scientific Inquiry has inaugurated or continued several important lines of investigation. The systematic survey of the physical and biological conditions of Lake Erie, begun in 1898 by Professor Reighard, has been continued. Dr. B. W. Evermann has made a biological survey of the waters of the Northwest; Dr. W. C. Kendall has continued his work on the fauna of the lake systems of Maine; and Dr. H. F. Moore has made a study of the physical conditions of Great Salt Lake, and has showed its absolute unfitness for maintaining any form of marine life.

The laboratories at Woods Holl and at Put-in-Bay have been occupied by an enthusiastic corps of investigators, and a building was rented at Beaufort, N. C., to serve as a temporary lab-

oratory. But with these matters readers of SCIENCE are well acquainted. The list of publications for the year numbers forty-six.

The work of the division under Mr. W. deC. Ravenel was the most extensive in the history of the Commission. More than one billion fry were distributed. Mr. Ravenel's report is illustrated with many photographic reproductions and plans of the twenty-nine stations. Mr. C. H. Townsend's statistical tables will prove of inestimable value to those who in the future may wish to follow the rise and decline of the different fisheries. The capture of one hundred and forty bowhead whales by the Pacific fleet in the Arctic Ocean produced an eventful, even if only temporary, elevation in the curve of decline of the whale fishery. It is with a feeling of sorrow that one reads of the slaughter of four thousand sea elephants on Kerguelen Island.

The articles published in the appendix are of both general and scientific interest. Several have a tropical flavor. The papers of Mr. W. A. Wilcox, Mr. C. H. Townsend and Mr. J. N. Cobb are mainly economic. Messrs. Evermann and Kendall have prepared an acceptable check list of the fishes of Florida. New genera and species of fishes from Porto Rico are described by Messrs. Evermann and Marsh. Dr. Moore gives an interesting account of his 'Inquiry into the Feasibility of Introducing Useful Marine Animals into the Waters of Great Salt Lake,' and Dr. Rathbun contributes 'A Review of the Fisheries in the Contiguous Waters of the State of Washington and British Columbia.' The scope of this paper is limited to the fishery questions of the region that are of international concern. While such papers have an immediate interest, their value really increases as time goes on, for they give a record of the more primitive biological conditions, without which it would be quite impossible in the future to determine the changes that have been wrought in the natural productiveness of a region by the occupancy of man.

H. C. BUMPUS.

Report on the International Cloud Observations. Prepared under direction of WILLIS L. MOORE, Chief of Weather Bureau, by FRANK H. BIGELOW, Professor of Meteorology. U. S. Department of Agriculture, Weather Bureau. Report of the Chief of the Weather Bureau for 1898–99. Vol. II. 4to. Washington, D. C. 1900. Pp. 787. Charts 79.

The Report on the International Cloud Observations, just published by the Weather Bureau, is one of the most detailed and elaborate studies of clouds that has yet been issued. Professor Bigelow, who has been in charge of the reduction of the observations, has not limited his investigations to the tabulation and simple discussion of the heights, velocities, and directions of movement of the different clouds, but has gone far into the thermodynamic and hydrodynamic problems which grew out of his study of the cloud observations. As he himself says in his preface: "In order to submit these results to a careful discussion, it has been necessary to prosecute a critical comparative study of several important theories heretofore proposed by meteorologists, so that comparison between observations and theoretical computations can be suitably carried out. Accordingly, a standard mathematical system has been constructed, including in a definite notation the constants, the thermodynamic and the hydrodynamic formulæ pertaining to the atmospheric physical processes and motions, by means of which the work of the several authorities can be reduced to one set of typical equations. The theories of the American and German schools of meteorology have been contrasted, and the results derived from them have been compared with the facts obtained from these cloud observations." This quotation may serve to give some idea as to the thoroughness with which Professor Bigelow has done his work. Indeed, the report is the most comprehensive and important of the Government meteorological publications of recent years.

There are in all fourteen chapters, the first two of which relate to the methods of taking the observations, and of computing the heights, directions and velocities. Chapters 3 to 7 contain summaries of all the observations made with nephoscopes and theodolites, and the discussions of these observations. The subjects treated in the last seven chapters are as follows: 'The Typical Local Circulations over the United States,' 'Diurnal Oscillations of the Barometric

Pressure in Relation to the Diurnal Winds,' 'A System of Fundamental Constants and Formulæ and Reduction Tables,' 'The Theory of Cyclones and Anticyclones,' 'Discussion of the Cumulus and Cumulo-Nimbus Clouds,' 'Reduction of the Pressure and Temperature Maps at Sea-level, 3,500-foot Level, and 10,000-foot Level,' 'The Amount of Heat that would Convert an Adiabatic Atmosphere into the State Actually Existing.'

The reading of this report cannot be lightly undertaken. Indeed, the very completeness of it and the elaborate mathematical discussions which find a place in it, will undoubtedly prevent many persons from attempting to find out what the volume really contains. We do not wish to be understood as saying that work of the sort that Professor Bigelow has here given us is unimportant, or out of place in a thorough study of the observations with which he has had to deal. Far from it. But we cannot help feeling, and feeling strongly, that the observers of the Weather Bureau, both regular and voluntary, and the public generally, should have the chief results of the international cloud observations in this country put before them in a simple, compact form. We hope that the chief of the Weather Bureau may look at this matter in the same light, and may perhaps sanction the publication of a Weather Bureau *Bulletin*, of say 100 pages, in which the results of Professor Bigelow's painstaking research, which are of most general interest, may be set forth.

Professor Bigelow is to be congratulated on the completion of this report, which stands on a far higher plane than most of the meteorological work published in this country.

R. DEC. WARD.

Plane Trigonometry. By DANIEL A. MURRAY, Ph.D., Instructor in Mathematics in Cornell University. New York, London and Bombay, Longmans, Green & Co. 1899. Pp. xiii + 301.

The author has aimed to 'avoid the extremes of expansion and brevity.' Only such topics are fully treated as make up the usual course in plane trigonometry. The thickness of the volume is largely due to the presence of an appendix of historical and other notes, a long list of exercises for practice and review, a table of answers, a four-place and a five-place table of logarithms of numbers, a five-place table of logarithms of the sine, cosine, tangent and cotangent, a four-place table of logarithms (augmented) of trigonometric functions, and a four-place table of values of trigonometric functions. These components constitute little less than half of the book. The other and larger half contains an unusually full exposition of principles. The composition is throughout careful and scholarly. While acquiring a knowledge of the elements as here presented, the student can hardly fail to become aware of the larger aspects of the science.

As regards arrangement and disposition of matter, there is, of course, always room for difference of opinion. Doubtless many teachers would for example prefer to have the notion of the radian introduced at an earlier stage; and there are not wanting reasons of some weight for preferring to present the general ratio definitions of the functions in connection with the conventional system of coordinate axes boldly in the *beginning*, instead of reserving this most commanding point of view, as is here done, for so advanced a stage as Chapter V. However, in things pedagogical, *quot homines, tot sententiæ.*

It remains to say that while paper and typography are good, the book deserves to be more substantially bound.

C. J. KEYSER.

COLUMBIA UNIVERSITY.

BOOKS RECEIVED.

A de Bary's Vorlesungen über Bakterien. Edited by W. MIGULA. Leipzig, W. Engelmann. 1900. Pp. vi + 186. M. 4. 60 Pf.

Outlines of Human Physiology. F. SCHENK and A. GRÜBER, translated by Wm. D. ZOETHOUT with a preface by JACQUES LOEB. New York, Henry Holt & Co. 1900. Pp. viii + 339.

Leçons de physiologie expérimentale. RAPHAEL DUBOIS. Paris, G. Carré and C. Naud. 1900. Pp. vi + 380.

SCIENTIFIC JOURNALS AND ARTICLES.

THE *Journal of the American Chemical Society* for December contains the following articles: 'The Production of Alloys of Tungsten and of

Molybdenum in the Electric Furnace,' Charles L. Sargent; 'A Method for the Rapid Determination of Carbon in Steel,' Robert Job and Charles T. Davies; 'Determination of Iron in Magnetite Ore by the Specific Gravity Test,' Joseph W. Richards; 'Irregular Distribution of Sulphur in Pig Iron,' Randolph Bolling; 'The Composition and Analysis of London Purple,' J. K. Haywood; 'Detection of Foreign Coloring-matter in Spirits,' C. A. Crampton and F. D. Simons; 'A Rapid Method for the Detection of Aniline Orange in Milk,' Hermann C. Lythgoe.

The December number (Volume 7, Number 3) of the *Bulletin of the American Mathematical Society* contains: 'Report of the October meet- of the Society,' by the secretary, 'On Linear Dependence of Functions of One Variable,' by M. Bôcher; 'Report on Groups of an Infinite Order,' by G. A. Miller; 'A Review of Ewing's Strength of Materials,' by Dr. Chas. Chree; 'A Review of Scheffer's Differential Geometry,' by J. M. Page; 'Notes'; 'New Publications.'

Popular Astronomy for January opens with an illustrated article by Miss Caroline E. Furness on the new 'Photographic Catalogue of North Polar Stars,' which has just been published from Vassar College Observatory. Professor Kurt Laves continues his discussion of the 'Adjustment of the Equatorial Telescope,' and Charles P. Howard his account of the 'Total Solar Eclipse.' Professor Francis E. Nipher's 'Positive Photography with special reference to Eclipse Work' is timely. Professor Herbert A. Howe, director of Chamberlin Observatory, of University Park, Colorado, begins his series of articles on astronomical books for the use of students. Professor Asaph Hall contributes a note on multiplication showing some peculiarly symmetrical results. The department of Astronomical Phenomena announces the eclipses of the coming year, and gives much space to news of comets, asteroids, etc. The number includes the usual general, spectroscopic and variable star notes, and W. W. Payne's second article on 'The Figure and the Attraction of the Earth.'

Nature announces that the Anthropological Institute will issue an anthropological journal to be entitled *Man*, which will appear monthly. Special attention will be given to the data concerning the origin of those forms of civilization which have become dominant.

SOCIETIES AND ACADEMIES.

BIOLOGICAL SOCIETY OF WASHINGTON.

The 330th meeting was held on Saturday evening, December 15th.

F. A. Lucas exhibited the under portion of the skull of a large specimen of the gar pike, *Lepisosteus tristoechus*, showing fracture and repair of the sphenoid. He stated that this was a good example of the fact that many animals could successfully recover from very severe injuries, since in the present case the breaking of the sphenoid must have entailed severe injury to and deflection of the entire cranium, and yet recovery had taken place. C. W. Stiles spoke of 'Some Tropical Parasites that may be introduced by our Returning Troops,' saying that the present conditions in China where troops were gathered from many parts of the world were particularly favorable for the interchange of parasites that were ordinarily confined to certain areas. The conditions in Manila were also favorable for attacks of parasites and their subsequent introduction into this country. The speaker described the various species of parasites that might be met with, their structure, mode of reproduction, the manner in which they entered the system and their effect upon it, illustrating his remarks by diagrams. But one or two of these species it was pointed out, were really to be apprehended, and as most of them were taken through the medium of drinking water, the danger could be almost entirely obviated by boiling the water.

E. W. Nelson presented a paper on 'The Caribbean Seal,' saying that while this was the first seal met with by the early explorers of the New World, and was an animal of considerable size and former extensive distribution, it was one of the least known of North American mammals and not accurately described until 1884. The various accounts of this seal, from the time of its discovery by the sailors of Columbus up to the present time were briefly no-

ted, the speaker then describing the habits of the animal as observed by him during a recent visit to the Triangles, in the Gulf of Campeche, to which the seal now appears to be restricted. They were sluggish and stupid, making practically no defense when attacked, and very easy to approach. While on shore they commonly laid on their backs, basking in the sun for hours, although the heat was so intense that iron exposed to the sun became too hot to handle with comfort, and dead seals soon had the epidermis so heated that the hair slipped off. Owing to the killing of these seals for oil, sold for lubricating purposes, their numbers had been greatly reduced, it being estimated that under one hundred were now living so that the extinction of the species would probably soon take place.

F. A. LUCAS.

TORREY BOTANICAL CLUB.

THE scientific program on November 28th consisted of a paper, soon to be printed, by Mr. Frederick H. Blodgett, on 'The Seed and Seedling of *Lilium tenuifolium* Fisch,' in which the seed characters were presented in detail, and with comparisons with those of *Erythronium*. Interesting differences were found in the size of the *Lilium* seeds about 93 per cent. of which germinated the small seeds as quickly as the larger, though with less vigorous subsequent growth.

On Tuesday afternoon, December 11th, the Club met at the Botanical Garden at Bronx Park. The program included a brief address by Professor Charles E. Bessey, a visit to the Garden conservatories with explanations by Dr. Britton, and the exhibition by Miss Anna M. Vail of valuable books recently added to the Garden library. Mr. R. M. Harper exhibited a very interesting series of specimens and photographs of plants from Georgia, and gave notes on their habitat and distribution. Dr. J. K. Small described a series of tree and shrub specimens from the south, with critical notes. Dr. D. T. MacDougal presented notes on the bulbils of *Lysimachia terrestris*. These bulbils are formed during the latter part of the season, in the axils of many leaves, and are morphologically branches. On completing their growth they pass into rhizomes. They are killed by freezing and desiccation.

Another paper was by Dr. M. A. Howe, 'Remarks on rare North American Hepaticæ.' The first hepatic discussed was *Riccia Beyrichiana*, a species which was discovered about seventy years ago 'between Jefferson and Gainesville, North America,' by the German traveller Beyrich, but which has of late been a subject of considerable doubt, inasmuch as it has not been seen since. Now, however, it has apparently been rediscovered by Mr. R. M. Harper, who found it during the last summer at Athens, Georgia, scarcely more than twenty miles from the locality where it was evidently first collected.

Dr. Howe also furnished a brief account of a collection of Hepaticæ made in the Yukon region by Mr. R. S. Williams—a collection of much interest, inasmuch as it contained one species which appears to be entirely new, one which has not heretofore been reported from this continent, five others new to the Alaskan region, and, besides these, two or three which have been rarely collected in America. The report on Mr. Williams's Hepaticæ is soon to be published.

EDWARD S. BURGESS,
Secretary.

THE ACADEMY OF SCIENCE OF ST. LOUIS.

AT the meeting of the Academy of Science of St. Louis of December 3, 1900, fifteen persons being present, Mr. William H. Roever, of Washington University, read a paper on 'Brilliant Points and Loci of Brilliant Points.' The paper gave the analytical conditions which define the brilliant point of a surface, the brilliant point of a space curve, the brilliant point of a plane curve and the brilliant point in space of two dimensions, when the source of light is such that the incident rays are normal to a given surface and the recipient is such that the reflected rays are normal to another given surface. Formulæ were given for the important special case in which the source and recipient are points. The paper also contained a general method for finding the equation of the locus of the brilliant points of a moving or variable surface or curve, together with a number of applications. Such loci may often be perceived when an illuminated

polished surface is rapidly moved, as when a wheel with a polished spoke is rapidly rotated. Another interesting example in loci of brilliant points is that of a circular saw which has been polished with emery in a lathe and thus received a great number of concentric circular scratches. The locus of the brilliant points of this family of scratches was shown in this paper to be a curve of the fourth degree. In the special case when the point source of light and the eye of the observer (the point recipient) are in a plane through the axis of the saw, the curve degenerates into a circle and two coincident straight lines. A photograph of the saw curve has been taken in which the optical center of the camera lens is the point recipient. Other interesting facts and a number of geometrical constructions were also given in this paper.

Three persons were elected to active membership in the Academy.

William Trelease,
Recording Secretary.

DISCUSSION AND CORRESPONDENCE.

THE ELECTRICAL THEORY OF GRAVITATION.

It is, perhaps, by the severe but impartial criticism of his work that the greatest of all possible obligations is laid upon the scientific investigator, for thereby his theories are purged of what may be incorrect or trivial, and that part of them which may be true is compacted and separated from what might otherwise hide its value, and cause it to be neglected.

Unfortunately I have been unable to profit as much as I felt I had a right to expect from Dr. Franklin's letter, Science, December 7th, as he has apparently been unable to find time for that careful examination and study which the subject, aside from the paper, demands. It is a matter of regret, also, in view of Dr. Franklin's admirable qualifications for dealing with the question, that he should have directed his criticism, in every single case, against theories which are the exact opposite of those which I hold, and which I have explicitly set forth in the paper referred to.

But though Dr. Franklin has with some slight lack of courtesy invited his readers to 'ignore' my remarks on the methods by which my theory was deduced, I shall not return the compliment by 'ignoring' his criticism, because it contains a number of very serious misstatements which should be promptly pointed out, as otherwise they may become sources of error.

To consider, first, his criticism of my paper, he says (par. 1):

"Professor Fessenden in a recent number of Science discusses the nature and velocity of gravitation. There is, no doubt, something of value in Professor Fessenden's suggestions and much that is new. However, the explanation of gravitation which Professor Fessenden offers is by no means so adequate as would appear from Professor Fessenden's discussion."

On careful perusal we find his reasons for making this statement to be three in number. In regard to the first he says:

"If we admit that the diminution of volume of the ether at each point is proportional to the resultant intensity of the electric field, then the part of the energy which depends upon diminution of volume cannot be separated in its effects from the part of the energy which depends upon the shearing distortion, inasmuch as both are proportional to the square of the resultant field intensity. Therefore a diminution of volume of the ether could not explain gravitation, but would only be involved in the explanation of ordinary electrical attraction and repulsion."

But, *so far from my theory implying a diminution of the density of the ether at each point proportional to the resultant field intensity, F, I have expressly stated that the change of density is proportional to* F^2, as witness the following extracts from my paper:

"Whilst the one which is a density must decrease with the *second* power of the corresponding intensity."

"And hence, as my experiments prove, the change in density is proportional to the *square* of the electric intensity."

As a matter of fact, even a cursory examination of my paper will show that the whole point of my argument rests on the fact that it is the second and not the first power which is involved. For the qualitative equation is

$$M/L^3 = T^2/L^2 \times M/LT^2,$$

i. e., density varies with the inverse square of voltivity and directly as compressibility.

Since, then, my theory calls for a change in density proportional to F^2 and since compressional energy varies as the second power of the compression, my theory makes the compressional energy vary as the fourth power of the electric intensity.

Dr. Franklin later says (last par.):

"If, however, the compressional energy were proportional to the fourth power of the resultant field intensity, then * * * gravitation would be provisionally explained."

Out of Dr. Franklin's own mouth, therefore, we have it that my theory provisionally explains gravitation.

As regards the second point, he says:

"Professor Fessenden, in his article referred to, speaks quite in general of the compression of the ether near a charged body, or ion, without localizing the distortion."

But this is not true. I have given the precise and exact distribution, par. 40, where I state,

"This change in density varies as the fourth power of the distance from the corpuscle."

I do not wish to complain, but no one cares to be continually misrepresented, and it is much to be regretted that Dr. Franklin was not able to note that I had covered the points he has criticised.

Thirdly, Dr. Franklin says (p. 889, 1st col. bottom):

"One might therefore expect that an hypothesis as to the constitution of matter which clears up the nature of inertia, even provisionally, would throw some light upon the nature of gravitation, but it does not seem to be so, and Professor Fessenden must needs say more from his point of view before we will be convinced."

But this is just what my theory *does* do. As I have pointed out elsewhere this is one of the very strongest points in favor of my theory, and in the paper criticised I have explicitly stated this, as, to quote (par. 41):

"The inertia of the atom is due to the electromagnetic inductance of the corpuscular charge, and gravity is due to the change of density of the ether surrounding the corpuscles, produced by the electrostatic stress of the corpuscular charge. *Mass and gravity thus bear a constant ratio.*"

Having thus answered all of Dr. Franklin's objections to my theory, I must now call attention to some very serious misstatements.

To take the first one. He says (p. 887, par. 2):

"Professor Fessenden claims to have derived *numerical functional relations* [the italics are Professor Franklin's] with the aid of his Qualitative Mathematics."

Now it is very wrong to say this. If I were to write an article in a scientific paper, stating that Dr. Franklin believed that the earth was flat, and after stating that this was believed by scientists to be impossible, 'on definite rational grounds,' and inviting my readers 'to ignore' his arguments, Dr. Franklin would justly consider that he had reason for complaint. But he would not have so much of reason as I have, for whilst Dr. Franklin has never, to my knowledge, published his opinions on this matter, I have, in no less than four papers, *explicitly stated views which are the exact reverse of those Dr. Franklin attributes to me.* In the very paper he is criticising I say (par. 5):

"Qualitative Mathematics, as its name signifies, is used, *not for the exact determination of numerical values*, but for the prediction and classification of phenomena."

I really could not put it any plainer. I do not see why Dr. Franklin makes his statement. He cannot point to *any* statement or *any* work which I have ever done in which I have tried to deduce numerical relations by means of qualitative mathematics.

So far from this being the case, I have frequently stated exactly the opposite. As instance the above quotation. Also in my paper on the 'Nature of Electricity and Magnetism,' *Phys. Rev.*, Jan., 1900. Also, in my paper in the *Electrical World*, of some years ago, I point out very specifically that this same numeral coefficient, which Dr. Franklin says I have overlooked, cannot be determined by Qualitative Mathematics, and I then go on to point out that since, this coefficient being of zero quality, we can always make it equal to unity by choosing suitable units, it is a matter of no consequence in discussing the *nature* of phenomena, however important it is as regards

the *quantity* of the action. *Thus, so far from overlooking it, I called attention to it, no less than four years before Lord Rayleigh did, in the article of his which Dr. Franklin quotes.*

So much for my theory. As regards my practice, one has only to read any of the papers in which I have used this method to see that I have never used other than experimental means to determine this unknown coefficient of zero dimensions. For example, in my paper on the nature of electricity, I first show that specific inductive capacity k is a density, and I then find, *by experiment*, what numeric k must be multiplied by to get the actual value of that density. Similarly, in the same paper, having shown that the magnetic coefficient a has the quality of hysteresis, I then proceeded, *by experimental means*, to find what the numerical relation between the two is.

A second misstatement is the following (p. 888, par. 4):

"Maxwell showed that the mechanical stresses in the dielectric tend to produce a diminution of volume."

There are no less than three mistakes in these lines. In the first place Maxwell never showed anything of the kind, and Dr. Franklin cannot refer to any passage in his writings where any change of volume, due to the electrically produced dielectric stresses is even hinted at. Second, the Maxwell stresses are incapable, as has been pointed out by several eminent physicists, of giving any diminution in volume, except on making assumptions not contained in Maxwell's theory or in his writings. Thirdly, the change in volume is not a diminution, but in the most general case an expansion, and only under certain conditions does it become a diminution.

Quinke had previously worked along that line, and found that some dielectrics expanded and others contracted, but did not give the law of the change. It was not until I had shown that contracting dielectrics behaved as negative uniaxial crystals in Kerr's phenomena, and did not obey the Maxwellian law, $1/\sqrt{k\mu}$ = velocity of light, whilst expanding dielectrics behaved as positive crystals and did obey that law; also that the compression depended upon the square of the electric intensity and the compressibility, and that mixtures and ionized compounds contracted whilst pure dielectrics expanded, that the phenomenon was exactly formulated, by me, as follows:

"All simple non-ionized dielectrics expand under electric stress, the change in volume being proportional to the square of the electric intensity, and inversely as the compressibility; they act as positive uniaxial crystals in Kerr's phenomenon, and obey Maxwell's law for the refractive index."

On p. 888, par. 2, he says:

"Physicists have known for many years" that attraction is to be attributed to ether energy, which decreases as the bodies approach each other."

Has this statement any basis of fact? Can Dr. Franklin adduce a particle of evidence to show that Hick's bubble theory, as developed by McAulay, or Newton's, or that theory of Bjerknes's which Larmor seems to regard with some favor, is more probable than the old corpuscular one (of le Sage? I write away from my books). As a matter of fact, can Dr. Franklin refer to the slightest evidence that ether is necessary to transmit gravitational force? And if Kelvin's value for the ether constants were correct, would this not be very improbable? And had it ever been shown that the ether has the properties requisite to do it, before I showed it, a couple of years ago?

Still one more point, and I mention this because I think Dr. Franklin has been a little unfair. In writing of the electrical theories of matter, he does it in such a way, no doubt unintentionally, as to convey the impression that the theories I have advanced are not original with me, but form a part of the common scientific stock of knowledge. For instance, in speaking of the 'electrical hypothesis of the constitution of matter.'

But it was the writer who first introduced the idea of the universal association of the electrical charge with matter. I believe that it is a fact that Dr. Franklin cannot refer to a single sentence in all scientific literature in which this theory was put forward, still less any proof given, prior to my papers of 1891 and 1892 in the *Electrical World* and SCIENCE, with their contained proofs. Prior to that date the ionic

charge had never been considered in connection with the atom save in relation to chemical and molecular effects.

The last statement I shall criticise is the following: He says (p. 888, 3d par.):

"It is now pretty well established that the ether energy having to do with electrical attraction and repulsion is dependent upon a sort of *shearing distortion* of the ether unaccompanied by any sensible diminution of volume, that this ether distortion is what is known as *electric field*, that the propagation of this energy constitutes *electrical waves*, and that the movement of the ether which comes into play during the establishment of this shearing distortion, or which comes into play while distortion at one place is relieved and distortion at a contiguous place is built up, is what is known as *magnetic field*."

Surely not!! So far from being established, Dr. Franklin cannot adduce the slightest particle of evidence for it. Though Maxwell and Lodge have used this theory, yet both Lord Kelvin and Professor J. J. Thomson have suggested exactly the opposite theory, and Heaviside has pointed out (Electromagnetic Theory, Vol. 1), that the theory which Dr. Franklin states is 'pretty well established' is at present as hard to reconcile with the facts as the other theory, so that the weight of authority would appear to be fairly evenly divided. And one of our greatest living physicists, J. J. Thomson, uses the opposite theory, of late exclusively. Moreover I have elsewhere pointed out that the variation of μ with the first power and of k with the second power is conclusive proof that the opposite theory is true.* If we chose to be unconvinced by this, then there is not the slightest evidence one way or another, and Dr. Franklin can add considerably to his already brilliant reputation by producing some evidence in favor of his statement.

REGINALD A. FESSENDEN.

* Those who are acquainted with my work on the nature of electricity and magnetism may remember that the proof that magnetism was a shear was based upon the following:

(*a*) The determination of the fact that either k or μ must be a density, thus confirming Williams's result.

(*b*) The demonstration of the fact that whichever one of the two k or μ is a density, must depend upon the first power of the corresponding force, whilst the other must depend upon the second power of the corresponding force.

(*c*) The experimental determination of the fact that μ varies with H whilst k varies with F^2.

A second proof was then indicated, depending upon the nature of the Lagrangian terms involved in the change of k and μ in elastic phenomena.

I have now to add a third. Briefly stated it is as follows: Since either k or μ is a density, then either H must be a shearing stress and F a velocity or *vice versa*. It is next shown that in the electric current we have a non-conservative system, and from quite general principles it is shown that it is the non-conservative system which must involve the velocities. And it is shown that under no circumstances could the equation expressing the amount of the I^2R loss be of the form it is if F were a shear, since in that case an operator which experiment shows is attached to an electric term would be connected with a magnetic term instead.

A BIBLIOGRAPHIC CATCH TITLE FOR THE YEARS 1900 to 1999.

IN a note published in SCIENCE, May 11th, I called attention to a bibliographic matter which I wish to return to again.

Some twenty years ago I adopted the plan of placing all bibliographic titles at the end of an article in a single list with authors' names arranged alphabetically and each author's papers arranged chronologically. As an essential part of the plan, the citation in the text consisted simply in giving the author's name and the last two figures of the year of publication preceded by an apostrophe. To avoid ambiguity, in case two or more cited papers were published by an author in one year, the abbreviated dates were followed by a lower-case letter used as an exponent. This plan has been kept up since then in the 'Contributions from the Zoological Laboratory at Harvard College.' Owing to its simplicity and the evident advantage which it gives the reader by acquainting him at once with the date of the paper cited, this plan has come into rather common use.

The apostrophe used to mark the omission of the first two figures of the year-date could not be used without ambiguity for dates subsequent to 1899, and I have consequently urged in the note

referred to that the apostrophe should be used to indicate an omitted 18, never an omitted 19. It then occurred to me that a comma might be similarly used to denote the omission of 19; but there seemed to be such important objections to this, that I dismissed the matter without further thought until, a few weeks ago, I received a letter from Mr. R. Pearl of the University of Michigan, in which he urged the desirability of adopting some method of abbreviation, and suggested the use of a period. There are, however, quite as serious objections to a period as to a comma. After some correspondence on the matter it has seemed to both of us that the *colon* so used would afford the best solution to the problem; but in order to avail ourselves of other possible suggestions, we desire to call attention to the matter in SCIENCE.

The signs hitherto considered and some of the more obvious objections to them are the following: The *comma* would be objectionable because in almost every citation two commas—one for punctuation, the other to mark the elision—would be brought together, and no proof-reader could be expected to accustom himself to the anomaly; thus in a recent publication, if the dates had been 1993, etc., instead of 1893, etc., the use of the comma would have given this undesirable result: "In Anurida, as in Orthoptera (Wheeler, ,93; Heymons, ,95[b]) and Lepisma (Heymons, ,97[a]), etc." The *period* is so commonly used as a decimal point, that .93 or .97[a], for example, would be misleading. It would clearly be of some advantage to have a character that should stand, like the apostrophe, on a line with the tops of the figures; but the various signs which usually have that position, as the asterisk, obelisk, etc., have such a fixed usage, as a means of referring to footnotes, that it seems unwise to employ any of them for this purpose. An *inverted period* would be open to the objection that a defective apostrophe could not be distinguished from it. The *dash* takes up too much room; the *hyphen*, though shorter, is not better in this respect than the colon, and has the disadvantage that, in the case of papers occupying more than a single year in publication, it now has to serve for omitted digits which might, or might not, be the figures 19, for which we should wish it invariably to stand.

So far as I recall, there is only one usage, except that of ordinary punctuation, to which the colon has been put that would be liable to interfere with its use for the purpose contemplated here. It has been used to separate the number designating a volume from that designating the first page of an article in that volume—a substitute for the letter *p*. Since in the proposed usage the colon would stand between the name of the author cited (not a number) and the last two figures of the year in which his paper was published, I think no ambiguity could arise. If, however, serious objections to the use the colon, or a better plan, occurs to any one interested in the matter, both Mr. Pearl and the writer would be glad to profit by suggestions communicated through SCIENCE or directly.

E. L. MARK.

HARVARD UNIVERSITY,
December 13, 1900.

ASTRONOMISCHER JAHRESBERICHT.

EACH year there is being issued under the editorship of Professor Dr. W. Wislicenus, from the press of Georg Reimer, an Astronomischer Jahresbericht, or annual review of all kinds of *astronomical publications*, including writings on geodesy and navigation if not too remotely connected with astronomy. This work is carried on under the supervision of the Astronomische Gesellschaft. The first volume contains the publications of 1899, and consists of xxiv + 537 pages, 8vo. This was issued in the spring of 1900.

In the interests of publishers, of readers and of the nation which he represents, the associate-editor for the United States desires to make the compilation and review of American publications on the above named subjects as complete as possible. To this end he invites authors and publishers to favor him with the title and place of publication of each book or article issued during 1901 and each subsequent year or a copy of the same if convenient that it may be reviewed for this purpose. The reviews are merely explanatory—not critical.

HERMAN S. DAVIS.

INTERNATIONAL LATITUDE OBSERVATORY
GAITHERSBURG, MARYLAND.

DIFFRACTION GRATINGS FOR GRATUITOUS DISTRIBUTION.

To THE EDITOR OF SCIENCE: Two or three years ago while engaged in some experimental work on the reproduction of diffraction gratings by photography, I devised a method of copying a Rowland 14,000 line to the inch grating, and silvering the copy, transforming it into a reflecting grating. The original was an excellent glass grating kindly loaned to us by the Johns Hopkins University. I was unable to get satisfactory copies with the bichromated gelatine but succeeded very well with albumin. I found this difficulty, however. To reproduce so fine a spacing it was necessary to use an exceedingly thin film, so thin, in fact, that the retardation of the light waves in traversing the 'bars' was insufficient to give spectra of any brilliancy. By thickening the film I was able to get a brilliant grating occasionally, but usually the lines ran together. It then occurred to me to silver the gratings, for the retardation by reflection is four times the retardation by transmission. The thin film failures, which I had thrown into a drawer as scrap plate-glass, were accordingly immersed in a chemical silvering solution, and when washed and dried were found to give brilliant spectra. One of these was exhibited by Professor Boys at a conversazione of the Royal Society about two years ago. Having about thirty of these gratings, which, while not as perfect as an original Rowland grating, being made on ordinary plate glass, are nevertheless suited for the ordinary purposes of the laboratory, I am desirous of placing them where they will do the most good. There must be among our many universities some physical laboratories which are not fortunate enough to have a good diffraction grating. I shall be very glad to distribute these copies to laboratories desiring them, as long as the supply holds out. I shall be glad if applicants will state whether the laboratory possesses a good spectrometer, and also the number of students engaged in the pursuit of physics. Some of the gratings are very good indeed, others quite poor as to general appearance, but all will give tolerably good spectra, and can be used for wave-length determinations. They will show the nickel line between the sodium lines in the solar spectrum very distinctly. Applications from high-schools will not be considered. Failure to receive any reply must be taken as evidence that the supply has been exhausted.

R. W. WOOD.

UNIVERSITY OF WISCONSIN.

NOTES ON INORGANIC CHEMISTRY.

ARGON AND ITS COMPANIONS.

ON November 15 a paper was read before the Royal Society by Professor William Ramsay and Dr. Morris W. Travers on 'Argon and its Companions,' which was a continuation of the previous papers of the same authors on the inert gases of the atmosphere. In the early summer of 1898 the discovery of neon and krypton was announced, and later a heavier atmospheric gas was found, to which the name xenon was given. At that time krypton and xenon were not obtained in a condition pure enough for the investigation of their physical constants.

The present paper deals chiefly with these three gases, which have been isolated and studied. By the evaporation of a large amount of liquid air a mixture of argon, krypton and xenon was obtained, the former largely predominating. This mixture was liquefied by liquid air and the three separated by fractional distillation, many times repeated. At the temperature of boiling air krypton has considerable vapor-tension, while that of xenon is scarcely appreciable. Neon was isolated from the first portion of gas escaping from boiling air. This consisted chiefly of nitrogen, which was then liquefied and a part of the liquid evaporated by passing through it a current of air. This gas, after the removal of the oxygen by hot copper, contained most of the helium and neon present in the air. After purification from nitrogen in the usual manner, the helium and neon were separated from the argon present by fractional distillation. To separate these gases was very difficult, but was finally accomplished by condensing the neon by means of boiling hydrogen. In this way pure neon was obtained.

A determination of the ratio of the specific heats of these gases showed that they are all monatomic. A number of the physical prop-

erties of these gases were determined, which are given in the following table:

	Helium.	Neon.	Argon.	Krypton	Xenon.
Refractivities (Air 1)	0.1238	0.2345	0.968	1.449	2.364
Densities of gases (O=16)	1.98	9.97	19.96	40.88	64
Boiling points at 760 mm	?	?	86.9° abs.	121.33° abs.	163.9° abs.
Critical temperatures	?	Below 68° abs.	155.6° abs.	210.5° abs.	287.7° abs.
Critical pressures	?	?	40.2 m.	41.24 m.	43.5 meters.
Vapor-pressure ratio	?	?	0.0350	0.0467	0.0675
Weight of 1 cc. of liquid.	?	?	1.212 gms.	2.155 gms.	3.52 gms.
Molecular volume	?	?	32.92	37.84	36.40

In the vacuum tube neon is extremely brilliant and of an orange-pink hue, and is characterized by multitude of intense orange and yellow lines; krypton is pale violet, and xenon is sky-blue.

The five elements clearly form a series in the periodic table, between the seventh and the first groups, that is, as a transition from the most negative to the most positive group. This is of the greatest interest, since in two recently published papers Ladenburg has given the atomic weight of krypton as about 59, placing the element between nickel and copper. It could thus find no place in the periodic table, as it is now understood. On the other hand, as determined by Ramsay and Travers, the elements find a natural place in the eighth group, as a transition from the negative series of the seventh group to the positive series of the first group.

J. L. H.

THE RESIGNATION OF PRESIDENT MENDENHALL.

Men of science everywhere will learn with deep regret that Dr. Thomas C. Mendenhall has been compelled by ill health to resign the presidency of the Worcester Polytechnic Institute. Fortunately President Mendenhall's health has only been injured by the great amount and responsible character of the work he has undertaken, and there is every reason to believe that after rest in Europe he will return prepared to continue work as important as that which he has already accomplished at the Ohio State University, the Imperial University of Japan, the U. S. Signal Service, the Rose Polytechnic Institute, the U. S. Coast and Geodetic Survey and the Worcester Polytechnic Institute. It is known to all men of science that, while occupying executive positions of great responsibility, President Mendenhall has carried out scientific researches of the utmost importance, and has taken an active part in all movements for the advancement and diffusion of science in the United States. He is one of the most efficient members of the National Academy of Sciences and of the American Association for the Advancement of Science, of which he has been president. He is now president of the American Metrological Society, chairman of the Massachusetts Highway Commission and a leader in many important scientific movements. As one of the editors of this journal since its reorganization six years ago his services have been invaluable. The grounds of President Mendenhall's resignation, the reluctance with which it was accepted and the great value of his work at the Worcester Polytechnic Institute is shown by the following letters. The letter of resignation, dated October 15, 1900, is as follows:

Hon. Stephen Salisbury,
President, Board of Trustees.
Worcester Polytechnic Institute:

Dear Sir:

I hereby tender my resignation as president of the Worcester Polytechnic Institute, to take effect on the first of July next.

I send this communication so long in advance in order to afford ample time for the selection of a suitable person to fill my place, and I hope I may be allowed to add a few words in acknowledgment of the continued and unvarying kindness which the board of trustees have shown me during my connection with the institute.

As far as I can now remember every suggestion I have made relating to the management of the institute has been approved by the board and every plan for its betterment that I have submitted has received its cordial, unanimous and hearty support. My personal relations with the members of the board have been, without exception, agreeable; everything that they could do to make my administration successful and my life in Worcester pleasant has been done, and I cannot give measure, however much I might multiply words, to the feelings of appreciation and grati-

tude with which I must always regard their considerate treatment of me during my term of service.

Naturally it is with great regret that I look forward to the early breaking off of a relation which has been to me so satisfactory. During the past year the condition of my health has been such as to cause much anxiety and this has been increasingly so during the past few months. The conclusion has been forced upon me that I must indulge myself in a long vacation or period of rest as free as may be from care and responsibility, too long to be for a moment considered compatible with a continuance of my connection with the institute. While thus compelled to sever this connection, there is no small consolation in the fact that I leave the institution at a period which may fairly be called the most prosperous in its history. Its numerical strength has not before been equalled; its next graduating class will be larger than any that has preceded it; its material equipment in the way of apparatus, machinery and other appliances for instruction has been greatly increased during the last few years and is now as nearly adequate as that of the best institutions of its class; during the same time new departments have been developed and specialization has increased; its internal affairs are directed by a relatively large, able and harmonious corps of instructors, and it is everywhere admitted to be in the front rank of the engineering schools of the country. Whatever progress towards this end has taken place during my administration, I wish to attribute to the generous support of the board of trustees, the cordial and enthusiastic cooperation of the faculty, the earnest and conscientious devotion to work which is and, I think, always has been characteristic of the student body, and, finally, the growing interest of the alumni, now nearly a thousand in number, scattered over the civilized world. The latter has been specially manifest on the organization of a number of new and active Alumni Associations, one of which is now to be found in almost every city of the first importance.

Under the continued fostering care of those who have so generously and unselfishly supported it in the past and with any one whom the board of trustees may select at its head, I shall confidently expect it to maintain its rank among the leading exponents of technical education.

I need hardly say that it will ever be a pleasure to me to further its interests in any way in my power.

I am yours faithfully,

T. C. MENDENHALL.

On December 20th in reply, the following communication was sent to Dr. Mendenhall by the committee on the part of the board of trustees:—

President T. C. Mendenhall, Ph.D., LL.D., Worcester Polytechnic Institute:—

Dear Dr. Mendenhall:

Your letter of October 15, 1900, resigning your office as president of the Worcester Polytechnic Institute, has been received by the trustees with expressions of profound surprise and regret. They deeply deplore the cause which alone necessitates this action on your part. They have been very slow and unwilling to admit the fact. In conference with you and at repeated meetings of the board, they have earnestly sought to find some way by which such a serious loss as your permanent separation from the institute might be averted. They are, however, compelled to recognize that your reasons leave them no option, either as respects your interests or those of the institute, and they therefore reluctantly accept your resignation as president of the institute to take effect, as you request, July 1, 1901.

In thus regretfully acceding to your request, the trustees wish to thank you for your thoughtful consideration of the welfare of the institute in giving them timely notice of your decision, and for the most courteous terms in which you have made it known to them. They also desire to communicate to you and to place on record their high and grateful appreciation of your laborious, devoted and successful service for the institute. During your administration, which in July will cover seven years, the institute has steadily grown in its productive endowment; in the size and value of its plant; in the excellence of all its facilities; and in the number of its faculty, instructors and students. New buildings have been erected and equipped. Large additions have been made to machinery and apparatus. The courses of study have been extended, revised and brought to the highest standards. The success, high rank and deserved fame of the institute have been greatly and solidly increased. All this has, in large measure, been due to your wisdom, faithfulness, tact and unwearying courtesy as president.

Nor would the trustees fail most cordially and thankfully to recognize the distinction which your ability as a scientific man, your skill and charm as a lecturer, and your ready, valuable and public spirited service of the city and the commonwealth, have given to the institute. In all these and similar directions your departure from among us will leave a vacancy not easy to fill. This adds to the pain with which, not only the institute, but also the city and the state, will lament your forced retirement on account of impaired health.

The trustees assure you that they fully reciprocate your expressions of personal esteem and of gratitude for the most amicable relations which have always existed between them and you. They earnestly hope that the rest and relief from responsibility to which you look forward will speedily result in your complete restoration to health and in fitness for increased usefulness and happiness, and to this end their good wishes will always follow you.

We remain, dear Mr. Mendenhall, on behalf of the trustees and with high regard, faithfully yours,

STEPHEN SALISBURY,
DANIEL MERRIMAN,
C. G. WASHBURN,
Committee of Trustees.

THOMAS A. WILLIAMS.

WE learn with much regret of the death of Professor Thomas A. Williams, assistant-chief of the Division of Agrostrology, U. S. Department of Agriculture, and a well known authority on botany and agriculture. He died at his home, near Washington, on December 23d, at the early age of thirty-five years. The high esteem in which Professor Williams was held by his colleagues is expressed in the following resolutions:

It is with feelings of profound sorrow and regret that we, the employees of the Department of Agriculture, learn that death has removed from among us our beloved companion and co-worker, Thomas A. Williams, a classmate and intimate associate of many of our number. His broad knowledge of scientific matters, his keen appreciation of nature, his kind and forbearing disposition has endeared him to all. He was known to his associates as an indefatigable worker and investigator.

Resolved, That in the death of Professor Thomas A. Williams, science and agriculture have suffered a great loss, and we, his associates, a dear friend, whose self-forgetfulness in his kindly consideration for the feelings of others, and uniform cheerfulness, often under conditions of severe physical suffering, revealed a lovable character of the highest Christian type. The loss to the Division of Agrostology is irreparable. In the performance of his official duties he had proved himself an excellent executive and an organizer of unusual merit and his relations with his associates in office were always such as to command the highest esteem and respect. He never shirked a duty, and however difficult the undertaking, the work performed by him was done most creditably. His loss will be felt most keenly by his associates in the division, and his memory will remain with them as one whose exemplary life and steadfastness of purpose they should strive to emulate.

Resolved further, That we tender to his bereaved family our heartfelt sympathy in their great loss and invoke for them the blessing of the Heavenly Father, who alone can heal the broken heart and give lasting comfort.

Resolved, That copies of these resolutions be sent to the family of the deceased and to the various daily and scientific journals.

COMMITTEE ON RESOLUTIONS FOR THE
DEPARTMENT OF AGRICULTURE.

WASHINGTON BIOLOGISTS' FIELD CLUB.

WASHINGTON, D. C., December 27, 1900.

Whereas, We have learned with sorrow and a sense of deep personal loss of the sudden death of our fellow-member, Thomas A. Williams.

Resolved, That the Standing Committee of the Washington Biologists' Field Club voices the feeling of every member in paying tribute to the memory of Mr. Williams, whose gentle and kindly disposition, charming good-fellowship and sterling qualities endeared him to every one of us. We recall with pleasant remembrances his interest in the work of the Club and its social relationships, as well as the many delightful hours spent with him in the field. We extend our heartfelt sympathy to Mrs. Williams in this the hour of her great bereavement, which we hope may be lightened by the thought that he passed away in the same peaceful and tranquil spirit in which he had lived.

Resolved, That a copy of this tribute be sent to the family of the deceased, and that it be published in *The Plant World*, of which he was an editor, and in SCIENCE.

By direction Standing Committee,
Washington Biologists' Field Club,
CHARLES LOUIS POLLARD,
Chairman.

THE AMERICAN SOCIETY OF NATURALISTS.

THE Society met at Johns Hopkins University, Baltimore, on the 27th and 28th of December. In addition to serving as a center for the important affiliated societies devoted to the natural sciences, the Society as usual had what may be regarded as two scientific sessions. The address of the president, Professor E. B. Wilson, printed above, was given at the annual banquet on Friday evening, and on Thursday afternoon a discussion, which will also be printed in this Journal, took place, its

subject being 'The Attitude of the State towards Scientific Research,' and the speakers being Professor H. F. Osborn, Dr. L. O. Howard, Professor W. B. Clark, Dr. B. T. Galloway and Professor William T. Sedgwick.

The business transacted by the Society included an appropriation of $50 toward the support of the university table at the Naples Zoological Station, and $50 to support a table at the Marine Biological Laboratory at Woods Holl. It was also voted that a committee of five be appointed by the incoming executive committee to select appointments to the university table at Naples, and to raise, if possible, additional funds for its support. A resolution was passed protesting against the duty on wax and other models intended for scientific research in educational institutions.

The most important business transacted by the Society was the consideration of its relations to the Naturalists meeting simultaneously at Chicago. The action taken was as follows: (1) Section 1, of Article IV, which limited the territory for holding meetings, was striken out of the Constitution and was transferred to the By-Laws. (2) This By-Law was temporarily set aside for the purpose of providing for the meeting of the Society in Chicago in 1901. (3) The Executive Committee was empowered to confer with those interested in the formation of a Western Branch of the Society.

Officers for the ensuing year were elected as follows:

President, Wm. T. Sedgwick; *Vice-Presidents*, L. O. Howard, L. M. Underwood, J. Walter Fewkes; *Secretary*, A. D. Mead; *Treasurer*, M. M. Metcalf; *Executive Committee from Society at Large*, G. H. Parker, J. McK. Cattell.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

A MEETING of the Council of the American Association for the Advancement of Science was held at Johns Hopkins University, Baltimore, at one o'clock on December 28th, with the president of the Association, Professor R. S. Woodward, in the chair. Professor C. S. Minot, the president-elect, was also in attendance.

The permanent secretary, Dr. L. O. Howard, made a report on the affairs of the Association, including the preparations for the meeting next August at Denver. Professor L. G. Carpenter has been in charge of the local arrangements, and has visited the officers of the Association and the leading scientific centers of the Eastern States in the interest of the meeting.

Local committees were appointed in a large number of places throughout the country to forward the interests of the Association in different regions.

A committee was appointed to inquire whether universities and other scientific institutions would set aside a week during the winter for the meetings of scientific and learned societies.

Miss Watson presented her resignation as assistant secretary of the Association, and was elected a life member in recognition of her long and efficient services.

247 new members were elected, which makes the total membership of the Association over 2,000.

The contract made by the Association with The Macmillan Company was presented. It carries out the arrangements made at the New York meeting, in accordance with which SCIENCE is sent free of charge to all members of the Association.

SCIENTIFIC NOTES AND NEWS.

WE expect to publish in early issues accounts of the scientific proceedings of the societies that met during Christmas week. In the meanwhile it may be stated that Professor W. T. Sedgwick, of the Massachusetts Institute of Technology, has been elected president of the American Society of Naturalists; Professor J. S. Kingsley, of Tufts College, president of the American Morphological Society; Professor W. H. Welch, of the Johns Hopkins University, president of the American Society of Bacteriologists; Dr. Erwin F. Smith, U. S. Department of Agriculture, president of the Society of Plant Morphology and Physiology; Dr. Frank Russell, of Harvard University, president of the Folklore Society; Professor Josiah Royce, of Harvard University, president of the American Psychological Association; Professor E. H. Moore, of the University of Chicago, president of the

American Mathematical Society, and Professor W. F. Clarke, of the U. S. Geological Survey, D. C., president of the American Chemical Society.

THE Presidents of sections for the meeting of the British Association for the Advancement of Science, which will be held in 1901, at Glasgow, commencing on September 11th, will be as follows:

Section A.—Mathematical and Physical Science, Professor Percy A. MacMahon, F.R.S.

Section B.—Chemistry, Professor Percy Frankland, F.R.S.

Section C.—Geology, Mr. John Horne, F.R.S.

Section D.—Zoology, Professor Cossar Ewart, F.R.S.

Section E.—Geography, Dr. H. R. Mill, F.R.G.S.

Section F.—Statistics and Economic Science, Sir Robert Giffen, F.R.S.

Section G.—Engineering, Mr. R. E. Crompton.

Section H.—Anthropology, Professor D. J. Cunningham, F.R.S.

Section I.—Physiology, Professor J. G. McKendrick, F.R.S.

Section J.—Botany, Professor I. Bayley Balfour, F.R.S.

AT the annual election of the New York Academy of Medicine on December 20th, the following officers were chosen: *President*, Dr. Robert F. Weir; *Vice-President*, Dr. Charles M. Dana; *Recording Secretary*, Dr. John H. Huddlestone; *Corresponding Secretary*, Dr. Louis F. Bishop.

AT the meeting of the Royal Statistical Society, on December 18th, a Guy medal was awarded to Mr. J. A. Baines, for his services to statistical science and a Howard medal and a prize of one hundred dollars to Dr. J. F. J. Sykes for his essay on 'The Results of State, Municipal and organized Private Action on the Housing of the Working Class in London and other Large Cities in the United Kingdom.'

PROFESSOR W J McGEE, of the Bureau of American Ethnology, has returned to Washington from a field trip through parts of Arizona, Sonora and Lower California. Outfitting at Phoenix about the middle of October, he traversed the sparsely settled plains southward to the International boundary at Santo Domingo, and thence to the village of Carborca. From this point the party worked down the sandwash of Rio San Ignacio (or Rio Altar) to the coast of the Gulf of California, where the Tepoka Indians lived until recently. Finding this tribe extinct, the expedition returned by a new route to Santo Domingo, and took the historical Yuma trail—now completely abandoned—to Rio Colorado. Here the territory of the Cocopa Indians was visited, and fairly extensive collections were for the first time made among them. Mr. DeLancey Gill, artist of the expedition, secured numerous photographs and sketches of the tribes and their territory.

DR. L. A. BAUER, in charge of the magnetic work of the United States Coast and Geodetic Survey, returned to Washington on December 15th, after having determined the sites of the base stations for the magnetic surveys in Alaska and in the Hawaiian Islands.

COLONEL FIVE, who was in 1896 sent by the Belgian Government on a scientific expedition to the far East, has returned home.

A MEMORIAL is being formed to commemorate the service to learning and letters of the late Professor Max Müller. The necessary steps are being taken to raise a fund, which, after providing for some personal memorial, such as a bust, relief or portrait, should be handed over to the University of Oxford, and held in trust for the promotion of learning and research in all matters relating to the history and archeology, the languages, literature, and religions of ancient India.

DR. P. H. KIRSCH died recently at Las Cruces, New Mexico. Dr. Kirsch received his doctor's degree from Indiana University about 1888. He was a teacher and naturalist of high rank, and was superintendent of schools in Franklin and other towns in Indiana and also in New Mexico. He is the author of several papers on fishes, one on the fishes of the Gila River, and another on the sturgeons.

FOR the purposes of the National Physical Laboratory, Queen Victoria has granted to the Royal Society, Bushey-house, Bushey-park.

THE annual report of the Superintendent of Disbursements shows an increased demand on the part of the public for Government publications and recommends that the issue of the monthly catalogue be enlarged, that permission

be granted to reprint documents of public interest and that a library be established, where not only national, but also State, municipal and foreign official documents may be preserved.

A TELEGRAM has been received at the Harvard College Observatory from Professor Kreutz at Kiel Observatory, stating that a comet was discovered by Giacobini at Nice, Dec. 20^d, 313 Greenwich mean time in R. A. 22^h 32^m 0^s and Dec. —22° 0′ 0″. Its daily motion in R. A. is + 6^m and in Dec. —8′. The discovery has been confirmed at the Lick Observatory, the comet having been observed by Professor Aitken on December 24th, 26th and 28th.

THE American Medical Association has appropriated $500 for researches. Applications for grants may be made during the present month to Dr. H. C. Wood, chairman of the Committee on Scientific Research, 1925 Chestnut street, Philadelphia.

THE Council of the American Chemical Society has authorized the establishment of a local section with headquarters in Kansas City.

AT a meeting of the Society of Arts, on December 12th, Professor Frank Clowes, chemical adviser to the London County Council, read a paper on 'The Treatment of London Sewage.' According to the London *Times* Professor Clowes said the present system of treatment consisted in straining or 'screening' off the larger solid matters and then mixing the sewage with solutions of lime and sulphate of iron; the chemical precipitate thus produced was then allowed to settle, together with the finer particles in the sewage, by sending the sewage slowly through parallel channels on its way to the river. The screenings or 'filth' were disposed of by being dug into the ground; the settled matter, or 'sludge' was sent in tank-steamers to be discharged out at sea; and the fairly clear 'effluent' passed constantly into the river at Beckton, Barking or Crossness. Although the condition of the river had been improved it had to be remembered that the effluent of the sewage after it had been freed from visible foul matter, still contained in invisible solution a large amount of putrescible substance, which might under suitable conditions lead to serious foulness in the stream. In 1893 the Main Drainage Committee of the London County Council started a large scale experiment on the bacterial purification of sewage. This experimental treatment had been considerably extended in its scope and the results pointed to a general conclusion that the settled sewage might be purified to a far greater degree by encouraging the spontaneous purifying action of the bacteria which were present in the sewage itself. The effluent thus produced, without the intervention of chemicals, remained free from foul putrefaction, and was able to support the life of fish. Professor Clowes proceeded to describe the bacterial treatment by means of coke beds filled three or four times per 24 hours. The maintenance of the purifying action was due to the presence of bacteria upon the coke surface and to the adequate aëration of these surfaces by frequently exposing them to the oxygen of the air. The chairman expressed the opinion that the lecturer had rightly emphasized the extreme importance of the subject as a question of sanitation. The late Professor Faraday once called attention to the foul condition of the river as evidenced by the fact that a piece of card which he threw into the water disappeared when it became immersed to the extent of only one inch. Professor Poore told them that they were altogether on the wrong track, that they ought to restore the fertilizing materials, now permitted to go to waste, to the land, without the intervention of water, but for the present, at all events, he was crying in a wilderness. The only practicable method for the present generation seemed to be to minimize the inevitable nuisance as far as possible. He wished that Professor Clowes had given them some information with regard to the cost of the processes he recommended as compared with the cheapness of letting it alone. He desired to know how much land and what sized tanks would be required for the bacteriological treatment of the whole of the London sewage. He should have been glad if the lecturer had told them whether there were not other methods of applying the activity of bacteria to the purpose in hand. It was desirable to know whether the admixture of the

salt tidal water had an injurious effect on the bacteria contained in the effluent.

UNIVERSITY AND EDUCATIONAL NEWS.

By the will of Henry Villard, Harvard University and Columbia University each receive $50,000. Among other public bequests is one of $5,000 to the American Museum of Natural History.

Ripon College dedicated its new science building, Ingram Hall, on December 18th. The keys of the building were presented to the trustees by the principal donor, Mr. O. H. Ingram, of Eau Claire, and received, with a fitting response by President Flagg. The principal address of the occasion was by Professor John M. Coulter, of the University of Chicago, on 'Some College Fallacies.' The building, which has been completed this fall, cost about $33,000, of which sum Mr. Ingram gave nearly $18,000 and Dr. Pearsons $5,000. The pleasant feature of the dedication exercises was a special gift by Mr. Ingram which wiped out a small debt remaining upon the building and allowed about $1,300 to be applied to the equipment. The building is three stories high and modern in all details of its construction.

The new building of the Medical College of Cornell University was formally opened on December 29th. Addresses were made in the afternoon by President Schurman, Dr. Stimpson, professor of surgery in the College, and Governor Roosevelt. In the evening, the new building was open to invited visitors.

Dr. D. K. Pearsons has given Colorado College the $50,000 promised some time since, and it is reported that he has given Northwestern University $30,000 for the erection of a woman's dormitory and $200,000 to an educational institution, the name of which is not to be made public during his life time. A press dispatch from Chicago accounts for about two and a half million dollars given by Dr. Pearson mostly to educational institutions, and it is estimated that the sum of about five hundred thousand dollars has been given anonymously. The details of the gifts are as follows:

Beloit College	$295,000
Chicago Theological Seminary	280,000
Colorado College, Colorado Springs	150,000
Mount Holyoke College, Massachusetts	150,000
Berea College, Kentucky	150,000
Lake Forest University	125,000
Whitman College, Oregon	120,000
Knox College	100,000
Drury College, Springfield, Mo.	100,000
Yankton College, South Dakota	100,000
Presbyterian Hospital, Chicago	70,000
Pacific University, Oregon	60,000
Carleton College, Northfield, Minn.	50,000
Fargo College, North Dakota	50,000
Pomona College, California	50,000
Fairmount College, Kansas	50,000
McCormick Theological Sem., Chicago	50,000
Chicago Young Men's Christian Assoc.	40,000
Olivet College, Michigan	25,000
Marietta College, Ohio	25,000
Sheridan College, Wyoming	25,000
McKendree College	25,000
Presbyterian Board of Missions	20,000
Grand Prairie Seminary, Onarga, Ill.	20,000
Anatolia College, Turkey	20,000
Woman's Board of Foreign Missions	20,000
Chicago Art Institute	15,000
Unknown College	200,000
Northwestern University	30,000
Colorado College	50,000
Grand total	$2,465,000

It is generally believed that Dr. Pearsons' anonymous gifts will bring the total close to $3,000,000.

Mr. W. J. Thompson, of Pittsburg, has given Washington and Jefferson College $50,000 for a library building and $10,000 for equipment.

The Hon. Roger Wolcott, formerly governor of Massachusetts, who during his life time gave liberally to Harvard University, has by his will bequeathed $20,000 to the institution.

The daily papers report that Mr. Alfred Vanderbilt has given $30,000 to the bi-centennial fund of Yale University.

At the meeting of the trustees of Ripon College, December 19th, President Flagg tendered his resignation. The administrative work of the college will continue in the hands of the dean of the faculty, Professor Marsh, as it has during the fall term.

Professor Arthur Thompson, of Oxford, has been elected Professor of Anatomy in the Royal Academy, London.